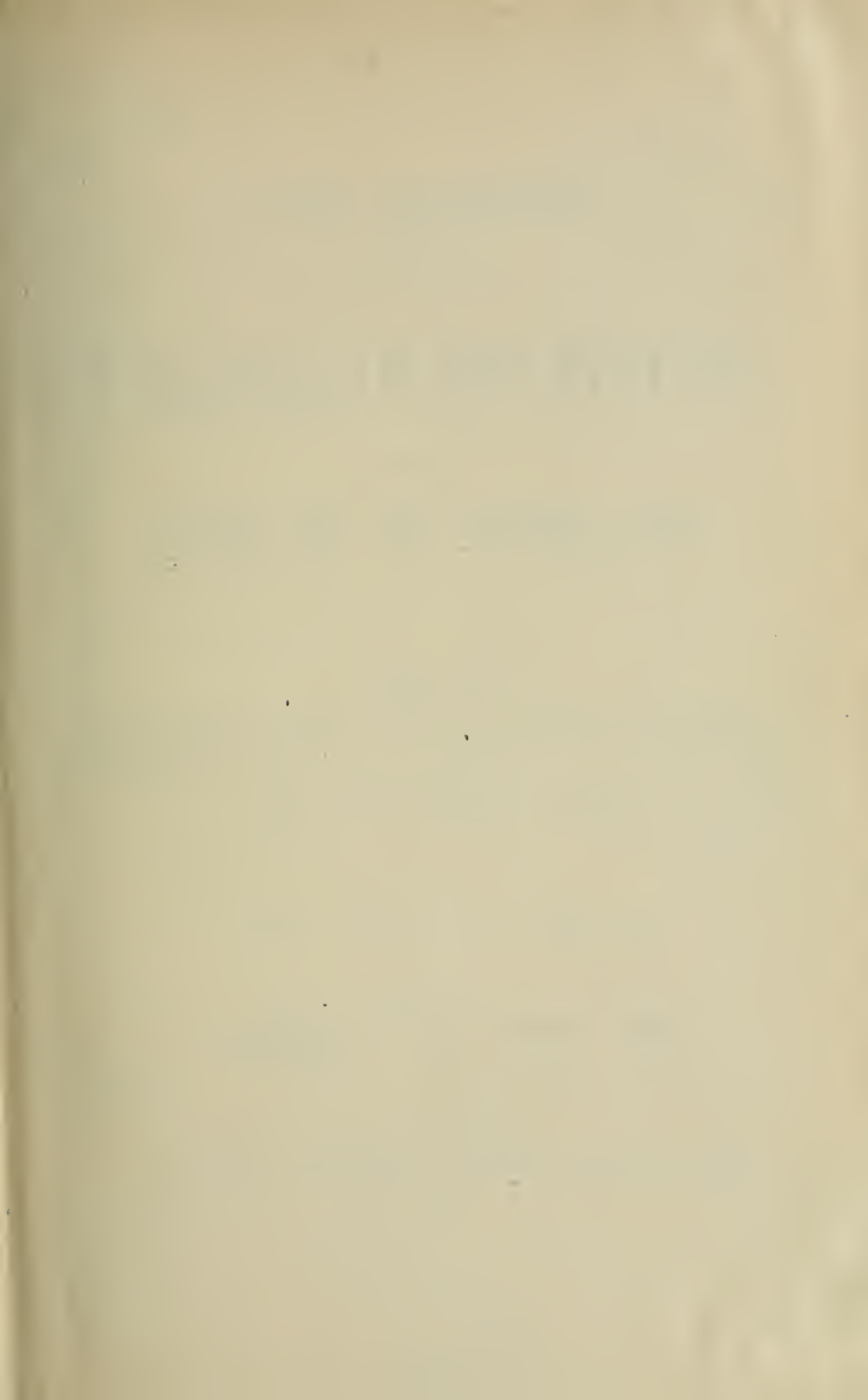




THE ROYAL CANADIAN INSTITUTE









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# THE JOURNAL

—OF THE—

# FRANKLIN INSTITUTE,

DEVOTED TO

SCIENCE AND THE MECHANIC ARTS.

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EDITED BY

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## PHOTOGRAPHY IN THE COLORS OF NATURE.

BY F. E. IVES.

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[*A Lecture delivered before the Franklin Institute, December 19, 1890.*]

THE lecturer was introduced by Prof. EDWIN J. HOUSTON, of the institute, and spoke as follows :

MEMBERS OF THE INSTITUTE AND LADIES AND GENTLEMEN :

I have already made two communications to the Franklin Institute on the subject of photography in colors. My object in lecturing upon it at this time is to go further into its history, to give such accurate knowledge of the subject as will make the force of my criticisms more readily apparent than heretofore, and to better demonstrate my own method. The substance of my lecture might have been more appropriately presented in the form of a paper to be read at a stated meeting of the institute; but a satisfactory presentation of a subject so complex and difficult could not be

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made in the brief space of time allowed for the reading of such papers, and I offer this excuse for presenting it as a lecture.

Heliochromy—meaning sun-coloring—has been settled upon as a name for processes of photography in natural colors, or in the colors of nature. There are two kinds of heliochromic processes. In one, the light itself produces the colors, by direct action upon the sensitive plate. In the other, light does not produce colors, but is made to regulate their distribution and combination. Some of the colors of the spectrum were imperfectly reproduced by a process of the first kind nearly thirty years before the discovery of the daguerrotype process. Seebeck, of Jena, in 1810, found that chloride of silver, after preliminary exposure to white light, is colored a brick-red by prolonged exposure to the red light of the spectrum, and a metallic blue by the blue light. After the discovery of the daguerrotype process, several experimentalists tried so to modify the preparation of the chloride of silver plates as to make them capable of reproducing all the colors of nature. In a photographic textbook, published so long ago as in 1853, I find the following statement: “Even the long-debated question of the reproduction of the natural colors by the agency of light seems on the point of solution. \* \* \* M. Niepce de St. Victor, from whose well-known character as an experimental philosopher much might be expected, has forwarded to London, as we understand, specimens of proofs in which every color is reproduced with a vigor and richness truly wonderful.” Similar announcements have been made since that time, but the best results ever actually shown were nothing more than interesting curiosities. Dr. H. W. Vogel,\* who recently had an opportunity to compare some of the latest and most talked-about of these “photographs in natural colors” with the original colored pictures from which they were printed (by contact), says:

“The original is one of those transparent window pictures in bright colors, brought into market by Grimme and

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\* *Anthony's Bulletin*, 1890, p. 325.

Hembel, in Leipsic, as a substitute for glass painting. It represents a Cupid with yellowish-brown hair and wings, and a small blue scarf around the waist, whose ends wave in the wind. He carries an arrow piercing two hearts of ruby color; between the knees he holds a quiver with yellow ornamented opening, and in his left hand the upper part of a large brown cross-bow, with blue and yellow mountings, the lower part of which rests, with the figure, upon an idealistic thistle blossom of red leaves. The stem is of the same color, and the plant shows fresh green leaves. The picture has a pale-blue background, and red, green and yellow ornamentation around the border in very pronounced colors. This border ornamentation affords an excellent means of comparison with the print. The latter, in opposition to the bright original, shows a greenish-gray, partly dark, ground. At first look, one recognizes readily that of all the colors only the red of the original has been distinctly reproduced. But it is not true to nature; it has a copper-red color, and differs decidedly from the vermilion and carmine red of the original. Besides this copper-red, only the blue of the scarf and the mountings of the cross-bow and quiver comes out as a very pale light-blue, with no natural resemblance. The black lines of the border decoration appear alongside of this as a violet-black. These are the tones which to some extent have a similarity of color, but with the other colors it is not so favorable. The yellow squares and green trapezoids of the border decoration appear neither yellow nor green, but have a grayish-red tone. The blue fields are not blue, but greenish-gray, like the ground. It is most singular that several parts are reproduced in red, which actually are not red, but brown yellow, as, for instance, the hair, the wings, the cross-bow, the thistle, etc. The green leaves in the print show no fresh color, and the red leaves of the blossom and the body of Cupid show only a pale flesh color. \* \* \* The resemblance of the new photographic pictures to natural colors is, therefore, not very favorable. Only two colors can be recognized distinctly in the copy, of which the red is the best; in a less degree the blue, which is weaker as



far as the picture is concerned. The blue in the ornamentation around the border, and all other colors, either have not been reproduced at all, or are entirely unlike the original. \* \* \* If I compare the sample before me with the pictures I have seen in 1867 of Niepce de St. Victor, Becquerel and Dr. Zenker, I must confess that those much older productions were richer in color, although the tones deviated likewise considerably from the natural ones."

According to Capt. Abney, the red end of the spectrum produces red by promoting oxidation; the blue end, blue, by its reducing action.\* Prof. Mendola† says: "It may at first sight appear improbable that the coincidence between the colors of the spectrum and the colors of the impressed film is a mere accident; but, although this is difficult to believe, I venture to think that it is an accidental coincidence and nothing more. \* \* \* In the best specimens of these photochromatic spectra that I have seen, the colors were certainly nothing more than approximations to the pure spectrum colors; and even in these spectra, some of the colored effect was due to the unaltered ground-color of the film in regions where some particular color had produced no action at all."

The process by which such imperfect results have been obtained is too slow to be applied successfully to camera photography, and the results are not permanent.

In view of all these facts, it would appear that there is no scientific basis for a belief that any material improvement can ever be made in this process, and that all so-called progress along this line is a delusion. It is true that some distinguished photographic writers continue to regard every new modification of this old process and every new result of experiment with it, as another step towards the photographic reproduction of the natural colors; but I have no doubt that if the same writers had lived 200 years ago, they would have regarded the production of new yellow-colored metal alloys as steps towards the transmutation of the baser metals into gold.

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\* *Anthony's Bulletin*, 1890, p. 307.

† *Chemistry of Photography*, p. 324.



In my opinion, the first step towards the solution of this problem was taken by Henry Collen, queen Victoria's painting-master, who, in 1865, invented a plan of composite heliochromy. His plan was based upon a false conception of the nature of color, and means for carrying it out were then unknown; but it was a bright idea, and contained the germ of a successful process. Collen's original communication of his idea appeared in the *British Journal of Photography*, October 27, 1865, and reads as follows:

"It occurred to me this morning that if substances were discovered sensitive only to the primary colors—that is, one substance to each color, it would be possible to obtain photographs with the tints as in nature by some such means as the following:

"Obtain a negative sensitive to the blue rays only; obtain a second negative sensitive to the red rays only, and a third sensitive to the yellow rays only.

"There will thus have been three plates obtained for printing in colors, and each plate having extracted all its own peculiar color from every part of the subject in which it has been combined with the other two colors, and being in a certain degree analogous to the tones used in chromolithography. Now, it is evident that if a surface be prepared for a positive picture, sensitive to yellow rays only, and that the two negatives, sensitive only to blue and red, be superimposed either on the other, and be laid on this surface, the action of light will be to give all the yellow existing in the subject, and if this process be repeated on other surfaces sensitive only to red or blue, respectively, there will have been produced three pictures of a colored object, each of which contains a primitive color reflected from that object.

"Now, supposing the first great object achieved, viz: the discovery of substances or preparations, each having sensitiveness to each of the primary colors only, it will not be difficult to imagine that the negatives being received on the surface of a material quite transparent and extremely thin, and that being so obtained are used as above—*i. e.*, each pair of superimposed negatives to obtain the color of the third—that three positives will be obtained, each represent-

ing a considerable portion of the form of the object, but only one primary of the decomposed color of it. Now, if these three positives be received on the same kind of material as that used for the negatives, and be then laid the one on the other, with true coincidence as to the form, and all laid upon a white surface, it will not be difficult to imagine that the effect would be, not only the representation of the form of the object, but that of its color also, in all its compounds.

\*   \*   \*   \*   \*   \*   \*   \*   \*

“Although the idea I have endeavored to express in words may be utterly worthless, I am unwilling to let it slip away without notice, as it may on the other hand contain a germ which may grow and bear fruit in due season.”

The language of some parts of this communication is ambiguous; but, taken all together, with due allowance for the writer's unfamiliarity with photographic technology, it clearly amounts to a suggestion to make three photographic negatives of an object—one by the action of red light, one by yellow, one by blue; to print from each pair of these negatives (superposed as one) a transparent positive having the color represented by the third negative, and to superpose on a white surface the three prints thus obtained.

It was not possible to carry out Collen's suggestion at that time, because there was no known process by which plates could be prepared which were sensitive to single colors only, and no photographic plates were sensitive enough to red and yellow to admit of the production of such negatives by exposure through selective color screens. Had it been possible to carry it out, the results must have been very imperfect, not only because the entire procedure is based upon a false and misleading theory of color, but also because superposing two negatives to act as one would double the intensity of such parts as represented white, gray or pale-colored objects, with the result that if the color prints were made to show all the details of the negatives, the finished heliochromes would show all bright colors as if mixed with equal parts of black pigment.

On November 23, 1868, Ducos Duhauron, of Paris, applied for a patent\* for a process which differed from Collen's only in the manner of carrying out the same idea. Like Collen, he assumed that the spectrum is made up of three primary color rays and mixtures thereof. He said: "My procedure rests on the principle that the simple colors are limited to three—the red, the yellow and the blue—the combination of which in divers proportions produces the infinite variety of shades in nature." Like Collen, he expected to solve the problem by superposing red, yellow and blue prints taken from negatives made by yellow and blue, red and blue, and yellow and red light. But, instead of using plates sensitive to single colors only, he proposed to use plates sensitive to all colors, and to prevent the action of color rays not wanted by filtering them out with color screens placed in front of the photographic objective or sensitive plate; and, instead of superposing two negatives to act as one, from which to make the color prints, he proposed to make two colors (two-thirds of the spectrum rays) act to produce each negative, which amounts to the same thing, and would not obviate the defect I have mentioned as resulting from the doubling of intensity on uncolored objects. He proposed to make one negative through an "orange" screen, calculated to absorb the blue light and transmit the red and yellow; one through a "violet" screen, calculated to absorb the yellow light and transmit the blue and red; one through a "green" screen, calculated to absorb the red light and transmit the yellow and blue.

It was no more possible to carry out this idea in Duhauron's way in 1868 than to carry it out in Collen's way in 1865. It is true, Duhauron tried to carry it out, and showed specimens of work, but the red and yellow rays did not act on his sensitive plates,† and he admitted, in a communication to the French Photographic Society,‡ that "the

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\* Class xvii, Sec. 3, Serial No. 83,061.

† Yellow pigments were photographed by the green rays which they reflected.

‡ *Photo. News*, 1869, p. 319.

production of good results will \* \* \* involve the manufacture of compounds which have not yet been created."

Soon after Duhauron showed his first specimens, Charles Cros, of Paris, published another modification of Collen's plan.\* Like Collen, Cros proposed to make one negative by the action of red light, one by yellow, and one by blue; but by exposing the sensitive plates through red, yellow and blue screens instead of employing plates sensitive to single colors only. Instead of superposing each pair of these negatives to make each color print, he proposed to make a green print from the negative made by red light, a violet print from the negative made by yellow light, and an orange print from the negative made by blue light. He also suggested that ordinary positive prints made from these negatives might be illuminated each by the kind of light which it represented, and the three combined by the aid of suitable optical devices so as to form a single picture, showing all the colors. Cros' plan, although it could not succeed because based upon the same false and misleading theory as that accepted by Collen and Duhauron, nevertheless possessed one important advantage over the preceding methods: it was free from the defect of doubling intensity on those parts of the negatives representing pale or uncolored objects. But this advantage would be lost again, in the production of green, violet and orange-colored prints, which will combine to reproduce yellows and blues only with a degree of degradation comparable to that produced by Duhauron's method.

On Dec. 3, 1869, M. Poirée, of Paris, in a communication to the Photographic Society of France,† expressed doubts concerning the correctness of Duhauron's and Cros' theories, and suggested that better results might be had by making a greater number of negatives—a separate negative for each spectrum region. He said: "The process which seems likely to succeed best is that in which the colors are analyzed by isolating successively each ray, or at least the rays of the

\* Described in *Photographic News*, Oct. 8, 1869, p. 483.

† *British Journal of Photography*, 1870, p. 26.



same shade. \* \* \* This analysis is difficult to make with colored glasses; it might be done, as by Newton, by monochromatic lighting and successive exposures to simple rays of the same shade. \* \* \* The synthesis is made by means of black positive images and rays of the same nature as those which produced the corresponding negatives. \* \* \* It will then only be necessary to place one above another the colored images so obtained, so as to form one virtually and really. It will be identical with the model, because it will be formed by the same rays, in the same relation of intensity." This plan also could not then be carried out because no photographic sensitive plates were sufficiently sensitive to yellow, orange and red spectrum rays.

In 1873, Dr. H. W. Vogel discovered that bromide of silver can be made sensitive to the less refrangible spectrum rays by treatment with certain dyes, and the subsequent discovery of other and better color sensitizers supplied the means for carrying out either Collen's or Poirée's idea.

Duhauron, one of the first to avail himself of these discoveries, made some practical progress, and, in 1876, abandoned Brewster's color theory and patented a modified process,\* based upon the observation that, while there appeared to be *seven* "principal" spectrum colors, three coloring substances would "serve to express them." The coloring substances he named for this purpose are blue, carmine and yellow, and he decided that, in order to make such a process reproduce the colors of nature, the negatives should be made by the action of orange, green and violet spectrum rays, which are complementary to the coloring substances. Some persons have thought that he had the idea of making negatives to represent primary color sensations; but this supposition is negatived, not only by the absence of any declaration to that effect, but also by the fact that orange does not represent a primary color sensation, either in fact or according to any theory recorded in the text-books, and the violet rays are not the ones which

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\* British patent, July 22, 1876, No. 2,973.

most powerfully excite the blue (violet) sensation. The plan was also utterly indefinite as regards the relative effect of intermediate spectrum rays, and Dehauron himself, owing to the fact that he never tried the method upon the spectrum, had no accurate knowledge of its capabilities. In his latest and "perfected" process (1878),\* he employed no plate sensitive to either red or orange light; one negative was made chiefly by yellow light, another by green, and the third chiefly by violet and invisible ultra-violet rays.

Albert, of Munich, also took advantage of the discovery of color sensitizers to try to carry out Collen's principle according to Duhauron's original plan. He was the first to make the color prints by the collotype process, which led to the use of the term "chromo-collotype."

In 1879, Cros† abandoned the idea that red, yellow and blue are primary spectrum colors, but still held that there are three primary colors and mixtures thereof, and that these primary colors are orange, green and violet. Like Duhauron, he decided to make negatives by light of these colors and prints in blue, red and yellow.

In 188-, Dr. F. Stolze, of Berlin, made a series of investigations, and tried to solve the problem by devising a procedure more in accordance with Young's theory of color.‡ He said: "Although the colors correspond with certain external processes in nature, there is also no doubt that color as such is nothing objective, but a subjective sensation, based upon the peculiar irritation of the visual nerves by those external proceedings. We can, therefore, only hope to produce a picture in natural colors when we are enabled to reproduce upon the same the proceedings which furnish to us the color impression." "The general idea of all colors being based upon the three principal colors, red, yellow and blue, is an erroneous one." "Theodore Young \* \* \* assumes that there are three kinds of nerve fibres sensible

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\* "Traité Pratique de Photographie des Couleurs," Paris, 1878, *Photo. News*, 1878, p. 115.

† *Bulletin of the French Photographic Society*, 1879, p. 23.

‡ *Anthony's Photographic Bulletin*, 1888, pp. 516, 555, 588, 647, 678.



to red, green and violet. Objective homogeneous light excites all three; but with red the first is excited strongly, the second and third weakly; with blue, the second and third moderately strong, the first weakly; with violet finally, the third strongly and the first and second weakly. If all three kinds of nerve fibres are equally strongly excited, the impression of white light will take place."

This theory, in accordance with which Dr. Stolze tried to devise a theoretical solution of the problem, is only partly correct, measurements by Clerk Maxwell and others having shown that the "red" sensation is neither affected by blue-green, blue or violet rays, nor the "blue" (violet) sensation by red, orange or yellow rays, nor the "green" sensation by red or violet rays. Neither is it the red rays that chiefly excite the red sensation, nor the violet rays that chiefly excite the blue (violet) sensation.

As a result of elaborate calculations, which, it must be said, could just as well have been made without any reference to Young's theory of color, Dr. Stolze came to the conclusion that if three suitable selective color screens were used in connection with color-sensitive plates, three negatives of the spectrum might be obtained, from which prints in cyan blue, carmine and yellow, if superposed, would reproduce the color effect of the spectrum. He did not show how to make selective color screens calculated to secure the right kind of negatives to carry out this idea, nor state what should be the form of the intensity curves in such negatives of the spectrum. He merely gave a table, showing on what parts of the spectrum each negative should fix color, and said: "If successful \* \* \* in selecting the color screens in such a manner that they will let the colors pass through which are called for in this table, one will indeed be able to reproduce a pure spectrum in this way." By further calculations, he was able to show that this plan, even if successfully carried out, would not insure the correct reproduction of mixed colors. He said: "All pure saturated spectrum colors will also be obtained quite satisfactorily in the reproduction, but the mixed ones only partly." "Oftentimes they have to become more or less

impure." "But the clearest lights and a number of mixed colors appear very unsatisfactory." He added: "The intelligent support of the artist can lend improvement," and recommended also the production of a fourth (ordinary) negative, to be used in combination with the others, to modify the effect, especially in the high lights.

This plan cannot be said to definitely represent the application of Young's theory of color, but it may be practically better than anything that that theory would indicate, if we leave out of account the suggestion of a fourth negative.

In 1885, Dr. Vogel, published a plan which is a modification of Poirée's.\* Like Poirée, he proposed to make a separate negative for each spectrum region; but, instead of using plates sensitive to all colors and exposing through selective color screens, or illuminating the subject by monochromatic lights, Vogel proposed to sensitize plates specially for each spectrum region, which would amount to the same thing; and instead of projecting the pictures with colored lights, he proposed to make as many pigment prints as negatives, each in a color complementary to the light which acted to produce the respective negative, and to surperpose them as in the Collen method.

There are no known dyes with which this plan could be carried out, and even if there were, it is, I believe, too complicated to be practicable.

In February, 1888,† I demonstrated a procedure based upon the assumption that, although there are more than three or five or seven primary spectrum colors, all of them—and in fact all the colors of nature—can be counterfeited to the eye by three type colors and mixtures thereof. This was not a new observation, and my plan did not differ very materially from that of Dr. Stolze, minus the complication of a fourth negative, except that it was more definite: and instead of merely publishing it as a suggestion, I found means to carry it out, and made a practical demonstration of it. I proved the process by photographing the spectrum

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\* *Annalen der Physik* (N. S.), xxvii, p. 130; *Photo. News*, 1887, p. 568.

† *Journal of the Franklin Institute*, 125, 345.

itself, employing compound color screens carefully adjusted to secure definite intensity curves in the spectrum negatives, so that they would make color prints which counterfeited the color effect of the spectrum when superposed. The adjustment of plates and screens to secure *spectrum negatives having definite intensity curves*, which, I believe, had never before been done, made all the difference between an indefinite and uncertain method and one definite and precise.

Promising results were obtained by this process, but I soon came to the conclusion, already reached by Dr. Stolze, that a process might reproduce the color effect of the spectrum, and yet not be capable of reproducing perfectly the compound colors. The solution of the problem was incomplete until I discovered a new principle, according to which such a procedure can be made to reproduce not only the spectrum, but also all the hues of nature.

This new principle, first stated by me in a communication to this institute on November 21, 1888,\* is that of making sets of negatives by the action of light rays in proportion as they excite primary color sensations, and images or prints from such negatives with colors that represent primary color sensations.

In order to understand this principle, I must explain that although the spectrum is not made up of three kinds of color rays and mixtures thereof, the eye is only capable of three primary color sensations—a distinction of the utmost importance, for the reason that the spectrum rays, which most powerfully excite a primary color sensation, are not the ones which represent the character of that sensation. The primary sensations are red, green and blue (violet); but it is not the red, green and violet spectrum rays that most powerfully excite these sensations. According to Clerk Maxwell, the orange spectrum rays excite the red sensation more strongly than the brightest red rays, but also excite the green sensation; the greenish-yellow rays excite the green sensation more strongly than the purest green rays, but also excite the red sensation; the yellow rays excite the red

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\* *Journal of the Franklin Institute*, Jan., 1889.

sensation as intensely as the brightest red rays and the green sensation as intensely as the purest green rays. Maxwell's diagram is a graphic representation of the result of careful photometric measurements of the effect of the spectrum upon these primary sensations.

1, 2, 3 are spectrum colors, which represent primary color sensations, because each excites one primary color sensation exclusively, and *a*, *b*, *c* are curves, showing the relative power of spectrum rays to excite the respective primary sensations.\* These conclusions are stated and indorsed in all recent text-books on color, and that eminent physicist, Lord Rayleigh, goes so far as to say that the theory is as well proved as the law of gravitation.

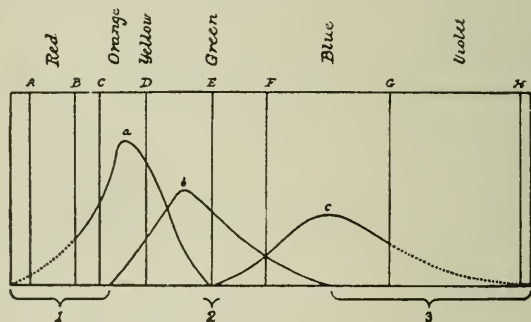


Fig 1

I emphasize this, because another theory of color has recently been proposed, which, although it has been circulated in the newspapers as if it had high scientific indorsement, is really inconsistent with established facts, and cannot, therefore, be true.

The carrying out of my new principle, according to Maxwell's measurements, therefore, involves the production of one negative by the joint action of the red, orange, yellow

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\* Taken separately, each of these curves is probably correct; but the three do not compound to represent accurately the relative visual intensity of different parts of the spectrum, because the curve of the green sensation is relatively too low, and that of the blue sensation too high; if the curve of the green sensation be drawn as high as that of the red, and the height of the blue curve be reduced one-half, they will compound to represent fairly a photometric measurement of the visual intensity of the spectrum.

and yellow-green rays, in definite proportions, to represent the red sensation ; one by the joint action of the orange, yellow, green and green-blue rays, in definite proportions, to represent the green sensation, and one by the joint action of the blue-green, blue and violet rays, in definite proportions, to represent the blue sensation.

Negatives of the required character can be made by exposing a cyanine-stained gelatine-bromide plate through a double screen of chrysoidine-orange and aniline-yellow of suitable intensity for the red sensation, a cyanine-erythro-sine gelatine-bromide plate through a screen of aniline-yellow of suitable intensity for the green sensation, and an ordinary gelatine-bromide plate through a double screen of crysophenine-yellow and RRmethyl-violet for the blue sensation. The plates and screens are correct when they will secure negatives of the spectrum showing intensity curves substantially like the curves in Maxwell's diagram. The negatives can also be made on certain makes of ordinary commercial gelatine-bromide plates of the most rapid kind, by the use of quite different color screens for the first two, but only with exposures of from five to fifteen minutes on well-lighted landscapes, aperture of objective *f. 12*.\*

In photographing objects in a changing light, landscapes, for instance, it is important that the three sensitive plates be exposed simultaneously ; and in order to accomplish this, I devised a triple camera, having three lenses so arranged in connection with reflectors as to bring all the points of view within a one-inch circle. With this camera, the production of sets of negatives of the required character is a simple and easy matter, it being only necessary to insert the plates, raise the flap until the exposure is made, take the

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\* I claimed that the production of such a set of negatives as I have described represented the application of a new principle in composite heliochromy. Some photographic *littérateurs* were disposed to sneer at such a claim ; but after leaving the matter open for discussion for nearly two years, I applied for a United States patent, and, in view of all the evidence, was granted exclusive right to make such sets of negatives.



plates out again, and, when convenient, to develop them together, in the ordinary way.

There are two ways of making the heliochromic pictures from these negatives. The first method does not produce a permanent picture, but a screen projection.

Lantern slides made from the heliochromic negatives and exactly reversing their light and shade must also represent the effect of the object upon the respective color sensations. One lantern positive, when seen by transparency in red light, reproduces the effect of the object upon the primary red sensation. Another, viewed in the same manner by green light, reproduces the effect of the object upon the green sensation. The third, viewed by blue-violet light, reproduces the effect upon the blue sensation. Evidently, the combination of these three images into one must form a reproduction of the object as seen by the eye, correct in form, color, and light and shade. Such a combination is effected by projecting the three pictures with a triple optical lantern, so that they exactly coincide upon the screen. The result is what we have been led to expect.

We have here a true solution of the problem of reproducing the colors of nature in a screen picture, dating from November, 1888. Previous to the publication of my new principle, it was assumed by Cros, Poirée and others, that if the projection method were employed, each picture should be projected by the same kind of rays as those which acted to produce it. In my method, as I have already stated, a picture made by the joint action of red, orange, yellow and yellow-green rays, but chiefly by orange, instead of being projected by a similar mixture of spectrum rays, is projected by red rays only. Similarly, the picture made by orange, yellow, green and green-blue rays is projected by green rays only, and that made by blue-green, blue and violet rays, by blue-violet rays only. That is the true principle, yet nothing of the kind had ever been suggested. The process is capable of giving results which are above criticism, except of that hair-splitting kind which applies also to the ordinary photographic process as a means of reproducing objects which have no color. The most serious objection to this method of solving



the problem is that its only commercial value would lie in its application to the illustration of popular lectures.

Dr. Stolze, who was one of the first to recognize the genuineness of this solution of the problem, doubted if, even in theory, color prints from the same kind of negatives could be made to furnish such a perfect solution. A year ago, I also believed that there were theoretical difficulties in the way of realizing a perfect process with color prints. Only recently have I succeeded in showing what relation the colors of the prints must bear to the colors of light used in projection, in order to perform exactly the same function and, under like conditions of illumination, secure equally perfect fulfilment of theoretical requirements.

In the projecting method, we build up the luminous image by adding light to light. White light is produced by the mixture of the three colored lights used for projection, and black by their suppression. But when we carry out the process to produce permanent pictures, the paper which may form the basis of the picture is itself white, and it is the shadows that are built up by the superposition of color prints.

Nevertheless, the color print has exactly the same function to perform as the lantern positive, *i. e.*, to absorb and suppress, by its shading, light affecting one primary color sensation. If we remove our three positives from the lantern, the screen is evenly illuminated with white light. If we then replace the one representing the green sensation, its shadows will absorb the green light, with the result that the screen bears a picture in the complementary color, pink, on a white ground. In the color-print method, we commence with a white surface, which corresponds to the fully illuminated screen, and the shadows of the color print representing the green sensation, when laid upon this surface, absorb the same kind of rays as the shadows of the positive in the lantern, and with the same result, a pink monochrome picture on a white ground. Superposing the other two color prints upon the first one on paper is like inserting the other two positives in the lantern. This explains why the primary sensations are represented by prints having shades

of the complementary (absorbing) color. It is the lights and not the shades of the color prints that represent the effect upon the respective primary color sensation. It is only necessary to use dyes that completely absorb red light but neither green nor blue-violet for the print representing the red sensation, green but neither red nor blue-violet for the green sensation, blue-violet but neither red nor green for the blue sensation, in order to obtain from my negatives a color print heliochrome that exactly fulfils all theoretical requirements, provided that it be examined in the same kind of white light that we obtain in the screen projections, by mixing red, green and blue-violet rays. The dyes mentioned by me in my paper of November 21, 1888 (prussian-blue, aniline-magenta and aniline-yellow), fulfil this requirement, and color-print heliochromes made therewith according to my instructions must, therefore, reproduce all the colors of nature under the conditions of illumination just stated.

We have, then, a theoretically perfect and, at the same time, practicable process of reproducing all the colors of nature in permanent prints from three negatives.

In order to obtain colors that would appear of exactly the right kind and shade in ordinary white light, it would be necessary to use dyes each of which completely absorbed all light affecting the color sensation which it represented, but no other. The colors would then be correct in ordinary white light, but would appear too dark, relatively to the white ground. In order to obtain colors that appear brighter in ordinary white light, dyes may be used which completely absorb only rays that excite chiefly single primary sensations and other rays in due proportion. The dyes proposed by me also fulfil this requirement, so that even in ordinary white light the degradation of a color is insignificant, except in the greens, where it is noticeable.

I have seen some of the results produced by the older processes of composite heliochromy, and others who have also seen them will, I am sure, bear me out when I say that the colors have invariably been not only untrue, but either very dull or else flat and patchy and wanting in the delicate

details and gradations of light and shade which characterize good monochrome photographs. All that showed bright colors resembled nothing so much as cheap chromos. In the composite helichromes by my process, which I show to-night, the colors are, as you can see, as perfect in detail and gradation as the monochrome shades of an ordinary photograph.

According to Captain Abney, the pictures produced by the silver sub-chloride process are "photographs in natural colors." Those which I now show are not so in the same sense, but they are something more and better than that—they are photographs in the colors of nature.

I have already spoken of a class of writers who still believe in the ultimate success of the silver chloride process, or something like it. It is not very surprising that men imbued with such a belief should be displeased with the idea of composite heliochromy, which is something short of a miracle; but the worst of it is, that they will not even take the trouble to make themselves familiar with the subject, and almost every utterance they make in reference to it is calculated to mislead the public and discredit true progress. I do not know how many times such writers have assured the public that composite heliochromy was based upon the same principle as chromo-lithography, and was merely the production of colored pictures by the aid of photography. Even Dr. Eder, a most able photographic chemist, is reported to have assured the representative of a London newspaper that my process was "chromo-lithography, in which photography is only an accessory!" Now, there really is such a thing as photochromy, which is carried out on the same plan as chromo-lithography, but it is no more like composite heliochromy than the Morse system of telegraphy is like telephony. In photochromy, it is only necessary for the photographer to make one negative of the object to be reproduced, and this negative contains a register of form and light and shade only. Composite heliochromy cannot be carried out with less than three negatives, which must contain a register not only of form and light and shade, but of color also. In photochromy, an artist is employed to regulate the

distribution of colors, according to his taste or judgment; in composite heliochromy, it is the light itself which regulates their distribution and combination, automatically, according to fixed and true scientific principles. Photochromy is an art; composite heliochromy, a science.

There are others who do make a distinction between photochromy and composite heliochromy, but whose statements are nevertheless too misleading to have any value. Only a few months ago, there appeared in one of the oldest and most pretentious of the photographic journals, an editorial article upon this subject, in which reference was made to "the three primary colors, red, yellow and blue," and all advance made upon the basis of true theory was discredited. Another, writing for an important periodical, said, "the red, yellow and blue" theory worked well enough in practice, and classed as an "advanced worker" one who had never got beyond that idea in composite heliochromy, or even contributed anything to its development. Dr. H. W. Vogel, taking advantage of the prevailing ignorance, even tried to make the readers of *Anthony's Bulletin* believe that I had claimed as my own something which belonged to him.\*

The frequency of such misrepresentation by writers, from whom the public has a right to expect something very different, is my justification for assuming the office of teacher and historian long enough to state the facts, which many people have wished to know, but could not discover by reference to current photographic literature.

In conclusion, for the benefit of those who would like to know why this process is not now in commercial operation, having been perfected in theory three years ago, I will say that, for various reasons, it is not practically available to one whose time is nearly all taken up with a business of a different character, and I do not expect to do much with it until I shall have completed preparations which will justify me in making it my chief occupation. In order to carry out the process in strict accordance with the theoretical requirements, means must be employed not only to secure three negatives and three prints, each of which is correct by

\* *Anthony's Bulletin*, 1889, p. 101.

itself, but each must bear also a certain definite relation to the others. A very little over- or under-exposure of any one color print, or a very little too much or too little of the color stuff in the film, will change the shade of delicate colors. Fortunately, there is a simple optical test by which such a defect can be detected without reference to, or knowledge of, the colors of the object photographed; but at present it is difficult to secure such harmony of parts when but little time can be spared to devote to the operation of the process.

Composite heliochromy must always remain a comparatively costly process, when carried out in a manner calculated to yield the finest results, and can most profitably be brought before the public in the form of optical lantern lecture illustrations, not with the triple lantern, but with transparent color-print heliochromes mounted as lantern slides. If the color prints are made by the Woodburytype process, such heliochromic lantern slides, infinitely superior to hand-painted ones, can be made in quantity at a cost not exceeding one dollar each.

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## ELECTRICITY: ITS PAST, PRESENT AND FUTURE.

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BY RALPH W. POPE,  
Secretary American Institute of Electrical Engineers.

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[*A lecture delivered before the Franklin Institute, December 16, 1889.*]

The lecturer was introduced by Prof. EDWIN J. HOUSTON, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

We can have no conception of that period in the physical history of the world when electrical manifestations were unknown. This once mysterious natural agency existed before the creation of man. Electricity, however, direct from the laboratory of nature has not been subdued and impressed into our service, in the same sense that we have for centuries utilized the power of wind; or the force of gravity through the medium of falling water. Natural electricity, as it appears to us in the form of lightning from the clouds,



seems to be wasted energy. We may have learned from our school-books that it clears and purifies the air, yet thousands of our people are compelled to live where it is extremely doubtful if the air they breathe is improved by its influence.

All of the earlier electrical experiments were confined to the domain of frictional or static electricity, a branch of the science which, although interesting and instructive, has been of little or no practical use to mankind. It is the field of applied electricity, its earlier and most important service in the transmission of intelligence which first demands our attention. The original invention by Volta, of the so-called "pile" made public in 1800, was the important step which led up to the electrical triumphs of the nineteenth century. This seems to have been the first discovery of that form of electricity which has since been known as the voltaic or galvanic current. It is not sufficient for our purpose, however, that we should simply be aware of the existence of a force. However interesting it may be as a subject of scientific investigation, its commercial value dates from that period when, in the hands of the inventor, it is led to perform some useful service. The well-known history of the steam engine may be cited as an example of the utilization of a force, the existence of which had been known for ages. So familiar were its manifestations by reason of its intimate association with domestic life, that it probably escaped elaborate scientific investigation until through the genius of James Watt, it received immediate recognition as a most important factor in the industrial field.

The æolipile of Hero, in the history of steam application, may be classed with the galvanometer of Schweigger, invented in 1820. Each was a manifestation of the existence of force, but not in such form as to be at that time commercially applicable. It is interesting to note that the steam turbine utilizing the principle invented by Hero is now coming into successful use for the driving of dynamos, its high speed being especially valuable in this class of work.

Taking the voltaic pile of 1800 and the galvanometer of

1820 we have a combination, which, at first glance, might seem to be the equivalent of the apparatus used for years in submarine cable service, and which is the sole reliance to-day, whenever the atmospheric conditions are unfavorable to the operation of the siphon recorder. Indeed, Ampère in 1820 had already suggested the employment of a battery, lines of wire, and galvanometers, for telegraphic purposes. There was this material defect, however, that existed in the early galvanometer, as well as the later magnetic devices; they were not adapted for operation by electricity, at any great distance. Dr. Ritchie exhibited before the Royal Institution in London, 1830, a model telegraphic apparatus based on the idea of Ampère. In this, a galvanometer and wire being used for each letter of the alphabet, the whole formed too complicated an arrangement for practical use. The fact that none of these early experiments were successful in producing signals at a distance was the stumbling block that baffled every effort of the early electrical inventors. The voltaic pile, made up of alternate discs of silver, zinc, and cloth saturated with acidulated water, although producing a suitable current was not exempt from serious defects, and while it could have been made to serve our purpose for practical telegraphy, it was superseded by the fluid cell in its various forms and combinations, which we may consider as having been virtually perfected before the invention of the electric telegraph. The simplest form of the voltaic cell may be looked upon as the germ from which has sprung up our vast and continually increasing electrical industries.

There was, however, another natural force which had long been known, but which, like electricity, had not been utilized. This was known as magnetism, but in its natural and so-called permanent condition, its earliest use in the form of the magnetic needle, as in the mariner's compass, seems to have been its only important practical application. In the form of load-stone, or magnetic iron ore, this wonderful property of magnetism had been known to mankind for ages. Like electricity, no satisfactory explanation can be given as to what it is, although we are familiar with its



laws and effects. Acting as a continuous force, like that of gravity, it could not be utilized in mechanics, until the relations of electricity and magnetism were investigated, and the wonderful discovery of electro-magnetism was made by Arago in 1820. We are indebted to William Sturgeon for the discovery, in 1825, that inductive magnetic effects could be intensified by forcing an electric current to pass round and round a bar of soft iron, and that this magnetic effect entirely disappeared with the cessation of the current. Electricity and magnetism thus joined forces in the electro-magnet and they are now studied and practised jointly. Returning to the simple voltaic cell, which, as was shown, may be taken as the basis of electrical work, it should be thoroughly understood that what is known as the electric circuit, must in all cases be made complete, in the fields of telegraphy, telephony, electric lighting and power transmission. This peculiarity of electricity is not readily comprehended. Its action may be compared with that of the ordinary belt used in driving machinery, the belt of a sewing machine, for instance. If this belt is continuous, its work may be satisfactorily performed, provided its tension is properly adjusted. If the belt is broken, its continuity must be restored before work can be resumed. So with the electric current, let the circuit be broken, no energy can be transmitted, and, therefore, no work can be done until the circuit is restored. If a belt is slack and slips on the pulleys, we lose energy; and in a similar manner, if a conductor of electricity is not properly insulated, only a portion of the energy is transmitted.

Now, we may also conceive of the electric current acting like a belt in its inductive effects. The belt and pulleys running in one direction brought into contact with another belt and pulleys in such a manner, as to drive them by friction, would cause rotation in the opposite direction. Thus an electric current over one wire will cause an induced current to flow in an adjacent parallel wire, but of an opposite polarity, or in an opposite direction, as is more popularly understood. There is one more important property of electricity and magnetism which it is necessary to

consider, before we are prepared to survey the electrical field as a whole. It is a very simple proposition, that if electricity produced magnetism, and through the action of magnetism we produce motion; that motion applied to a magnet, under similar conditions would produce electricity.

This discovery by Faraday, of what is known as magneto-electricity, was not immediately appreciated in its final bearing upon electrical development as we see it to-day. Its principle was briefly that the movement of a permanent magnet in close proximity to a coil of insulated wire produced in the latter, an electrical impulse. This idea was developed into the magneto-electric machine, which, beyond its utilization, to some extent for medical purposes and electro-plating did not come into immediate use, although it was important as being the foundation of the dynamo of to-day, in which the permanent magnets have been superseded by the electro-magnet. It was discovered by Farmer, Siemens, Wheatstone, and Varley, at about the same time, in 1866, that the residual magnetism in the machine was sufficient without the use of permanent magnets.

The history of electricity is more complete than that of any other similar subject, therefore, we are able to trace the development of various inventions, credit for which, however, has, no doubt, been frequently misapplied. In treating of electricity in the past, we should take it up at that period when, through the medium of the electric telegraph, it served a purpose not previously met by any device known to man—that of communicating intelligible signals instantaneously between points separated by distances of hundreds or even thousands of miles. We have seen that as early as 1820, using the pile of Volta, and the galvanometer of Schweigger, we were on the eve of a great invention, yet it remained for a bright practical mind to overcome that obstacle which proved insurmountable to those who had already conceived the possibility of an electric telegraph.

It is through this gradual evolution of invention that the patient work of the early investigators is too frequently buried beneath the laurels which are placed upon the brow of him whom the world recognizes as the successful inven-

tor. We may honor Morse without disparaging the work of Arago, Sturgeon, or Henry, to whom he was indebted for the electro-magnet, without which his electric telegraph was a mere vision. The struggles of Prof. Morse with poverty, the perseverance with which he labored to secure a trial of his invention, the discouragements he encountered even from his friends are not only familiar to us, but we know it to be too frequently the experience of pioneers who are in the van. It is this very qualification of perseverance, however, that is frequently lacking in the temperament of the deserving inventor, and the field which he planted is reaped by other hands. The crude and cumbersome apparatus, originally used by Morse, has been gradually simplified and perfected, until it stands to-day, and will probably continue to stand, without a rival, for simplicity and effectiveness. The electro-magnet, originally used by Morse, was not adapted for use on circuits of a sufficient length to make his telegraph a commercial success. Through Dr. Gale he learned from Henry's experiments that the strength of the magnet could be increased by winding additional layers of wire upon it. It appears, however, that the same size of copper wire was used, both on the line and for the magnets. This necessitated the use of about 180 pounds of wire, in order to produce the same magnetic effect that we now obtain with about half a pound of fine magnet wire. In order to secure sufficient power to operate his embossing apparatus, he found it necessary to interpose what has since been known as the relay magnet, in which the main circuit was used to repeat the signals on a local circuit, and thus produce the necessary effort to mark the paper with a metal stylus. This ingenious device was a necessary element in the successful operation of the Morse telegraph up to the time when operators through long practice, had begun to read the signals by sound. Although the Morse recording register of the past is practically obsolete in commercial telegraphy, the relay and local battery still remain in service, the power required for the sounder being necessary in order to obtain the best practical results. So long as this feature is retained, and the alphabet of the distinguished inventor

remains in use, the telegraph should be known, as it is universally called to-day, the Morse system.

The more recent inventions which are identified with it, the duplex of Stearns, the quadruplex of Edison, and the multiplex of Delany, are all improvements in the direction of economy; that is they have increased the capacity of each wire, so that a much larger volume of business can be transmitted and the earning capacity of the plant increased accordingly. Each of the additional circuits thus created—phantom wires, as they are termed—requires the attendance of a skilled Morse operator at the transmitting, as well as the receiving station. This class of labor is considered by the companies as highly paid, but, as a matter of fact, the natural intelligence required, and the length of time devoted to attaining proficiency, is certainly entitled to reasonable compensation. The practical success of the Morse system, naturally stimulated the efforts of other inventors, and there was issued, in 1846, a patent to Royal E. House, for an improvement in electric telegraphs. This was a type-printing instrument; and, notwithstanding all that has been accomplished since that time, it must still be considered a masterpiece of ingenuity; and its practical perfection for its intended use, the mechanical skill displayed in making it a serviceable and durable machine for actual work, the House printer, as it was familiarly called, may fairly be considered an extraordinary invention. A successful trial of this machine was made in 1849 on the line between New York and Philadelphia. For speed and accuracy it was unrivalled in its day, but owing to the rapidity of the electrical pulsations essential to its operation, it was not adapted for use on the long circuits which the geographical conditions of the country rendered necessary. It gradually went out of use about 1860, being partially succeeded by the type-printing instrument of David E. Hughes, another beautiful invention introduced in 1857, which at one time seemed likely to largely replace the Morse apparatus. Some of the features of the House and Hughes printers were blended in the combination printer by the late George M. Phelps, who, subsequently invented the motor printer.



now used to a limited extent between New York and Boston, and New York, Philadelphia and Washington.

The Bain system, used by one of the rival telegraph companies in this country, forty years ago, was a chemical telegraph, a dot and line alphabet similar to the Morse being used, the current however, instead of producing a mechanical effect, was forced to pass through a sheet of moving paper, moistened with a chemical solution, upon which the characters appeared in dark blue lines. The only vestige of this invention, which has survived, is the occasional use of the chemically prepared slip in systems similar to the Wheatstone automatic telegraph, by which signals are transmitted at the rate of 500 words per minute, from perforated slips previously prepared. This system is being extensively introduced by the Western Union Telegraph Company, its principal merits being the increased capacity of a wire between cities where a large volume of business is transmitted, and the possibility of dispensing with the highest grade of operators. Messages are copied from the slip by an operator using a typewriter. It will be seen that electricity in the past has found its field in the commercial world as a conveyer of signals, convertible into intelligible communications as rapidly as the most expert penman could write them. Although the electrical action is instantaneous, there are certain conditions which continue to cause occasional delay and annoyance in the transmission of telegrams. The pressure of public opinion as voiced by the press has always been in the direction of lower rates, rather than toward prompt and accurate service, and the natural consequence has been a tendency to economy, which has not always or usually been conducive to the best results.

What electricity may do under proper conditions will, perhaps, be most strikingly shown by a brief reference to its public service for fire-alarm purposes. Nowhere is the value of time more forcibly shown than in the management of the fire telegraphs in our principal cities. Every precaution has been taken to remove all possible chances of failure, and there can be but one opinion as to the import

ant service thus rendered by electricity. The adaptability of the telegraph to this work was soon appreciated, and the active brain of Moses G. Farmer soon devised a practical method of utilizing electricity for this purpose. The simple break-wheel with its fixed characters, revolved by clock-work, was the foundation of the system, but there has been grafted upon it in later years a most elaborate combination of repeaters, gongs and various apparatus, forming altogether a most interesting collection of electrical machinery, which is not so generally appreciated as it should be. In this service we see electricity combined with highly trained men and animals, ever on the alert to aid, in its most important mission, for the safety of life and property. Everything must give way to the flying steamer. The autocratic truck driver must haul to one side, and the district messenger, with his important telegram, must hurry for once, to get out of the way.

\* In the department of signalling, electricity in the past has had no successful rival. Its various duties in the modern factory, hotel, office and residence, its still closer relation to all of us, through the "stock ticker," the messenger call-box, the burglar alarm, the annunciator, and the telephone, are so familiar that it is merely necessary to mention them to remind you of their value.

Electricity, as utilized by us to-day, has expanded into a much wider field, but one that has long been held by other interests and is viewed as a competitor, an enemy, or an interloper, according to the interests of those whose opinions we listen to. Its use in artificial illumination is perhaps the most important, and certainly the most conspicuous. Its development for this purpose, however, has been seriously retarded, owing to the lack of any sufficiently economical method of producing electricity. The arc lamp may be said to have slumbered from the date of discovery by Sir Humphry Davy, in 1808, until the invention of what is known as the Jablochkoff candle, in 1877. Its commercial possibility was due to the perfection of the dynamo machine by Siemens and Gramme, working independently, in 1871. The arc lamp had, however, been in practical use for light-



house purposes since 1858; the necessary current having been supplied from the magneto generator, based on the principle already referred to.

[To be continued]

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## THE CONTINUOUS GIRDER—VARIABLE MOMENT OF INERTIA—MOMENT AND LOAD COEFFI- CIENTS—GRAPHICAL METHOD.

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BY C. H. LINDENBERGER.

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With such a problem as this, all that can be reasonably hoped for to make it practical as well as useful, is that some simple graphical method may be invented in time. The case will then resemble some well-known problems in harbor surveying, or the famous problem of the Howe truss strut diagonal, which no one ever thinks of computing analytically. Most of the graphical methods known at present, however, are quite as complex and tedious as the purely analytic method.

The present article grew out of an attempt of the writer to invent a graphic method that would follow up and explain Prof. Howe's "Continuous Girder," the best, and in fact the only work on this subject in the English language. It was discovered, however, that the objection of graphical complexity and tedium was too strong to be overcome, and that the problem must be re-stated in order to simplify the graphical method. Let the reader judge whether this has been accomplished.

In order to anticipate criticism, it would be, perhaps, better to demonstrate that the results obtained are rigorously identical with those of Prof. Howe, as well as to deduce the new Theorem of Three Moments from the differential equation of the elastic line.

Let us begin by a few definitions :

Let  $\theta$  equal six times the modulus of elasticity multiplied by the moment of inertia. It will be noticed that the symbol  $\theta$  is used where Prof. Howe only uses the symbol  $I$ , the

latter referring only to the moment of inertia; but it should also be borne in mind that, if  $E$  is the modulus of elasticity, then the latter must always be a multiplier of  $I$ , except when it disappears as such by cancellation, because it is supposed constant, which is the usual and convenient hypothesis.

$\varepsilon_v$  and  $\varepsilon_{v-1}$  are the horizontal coördinates, whose origin is the  $r^{\text{th}}$  support, of two points in the  $r^{\text{th}}$  span, between which  $\theta$  is constant. The particular value of the latter is denoted by its subscript, which is  $(v-1)$ .  $\varepsilon_{v-1}$  is always less than  $\varepsilon_v$ , and the space between these points may include the whole or any less space over which  $\theta$  is constant. This is equivalent to giving the option of dividing a portion of the span over which  $\theta$  is constant, into two or more parts, when it is necessary; and is an option that Prof. Howe does not give, but which evidently would not change the results obtained.

Hence, the subscripts  $v$  and  $v-1$  do not require any rigid, once for all, system of enumeration of partial lengths to be adopted, and any portion may be subdivided at pleasure.

By Prof. Howe's definition, the value of  $\theta$  at the right end of the  $r^{\text{th}}$  span is denoted by  $\theta_r$ ; where the subscript  $r$  refers to the span and not to the number of parts. In the same manner,  $\theta_{r-q}$  would refer to the value of  $\theta$  at the right end of the  $(r-q)^{\text{th}}$  span. This is a refinement that is necessary in his analysis, but not in mine.

$l_r$  is the length of the  $r^{\text{th}}$  span.  $P_r$  is a weight on that span, and  $a_r (=k_r l_r)$  is its distance from the left support.

Let  $u_v$  be any function of  $\varepsilon_v$  and  $u_{v-1}$  a similar function of  $\varepsilon_{v-1}$ , then make

$$\frac{u_v - u_{v-1}}{\theta_{v-1}} = \Delta u_v \quad (1)$$

The sign  $\Delta$  indicates that the difference of the functions is to be divided by  $\theta_{v-1}$ . The function is always written in terms of  $\varepsilon_v$ , as follows:

$$\text{Let} \quad \frac{\Delta 2\varepsilon_v^3}{l_r^2} = \gamma_v \quad (2)$$

$$\text{Let } \frac{J(3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3)}{l_r^2} = \lambda_v \quad (3)$$

$$\text{Let } \frac{J(6 l_r^2 \varepsilon_v - 6 l_r \varepsilon_v^2 + 2 \varepsilon_v^3)}{l_r^2} = \tau_v \quad (4)$$

The ideas expressed by these last three symbols will become apparent when the graphical method is reached. To prevent misapprehension and show the conciseness of the symbol expand the definition of  $\lambda_v$ , thus

$$\lambda_v = \frac{(3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3) - (3 l_r \varepsilon_{v-1}^2 - 2 \varepsilon_{v-1}^3)}{l_r^2 \theta_{v-1}} \quad (5)$$

Suppose there were a number of successive differences of the function  $u$ , each divided by its appropriate value of  $\theta$ , to be added; the subscript of the first being  $n-1$  and of the last being  $m$  then the series would read as follows:

$$\begin{aligned} & \frac{u_n - u_{n-1}}{\theta_{n-1}} + \frac{u_{n+1} - u_n}{\theta_n} + \text{etc.}, \\ & + \frac{u_{m-1} - u_{m-2}}{\theta_{m-2}} + \frac{u_m - u_{m-1}}{\theta_{m-1}} \end{aligned}$$

Then by inspection we may write

$$\begin{aligned} \sum_{v=m}^{v=n} \frac{u_v - u_{v-1}}{\theta_{v-1}} &= - \frac{u_{n-1}}{\theta_{n-1}} + \frac{u_m}{\theta_{m-1}} \\ &+ \sum_{v=m-1}^{v=n} u_v \left( \frac{1}{\theta_{v-1}} - \frac{1}{\theta_v} \right) \end{aligned} \quad (6)$$

In using this equation, to transform those of Prof. Howe, we will in general have  $u_{n-1} = 0$  and  $u_m =$  value of function when  $\varepsilon_v = l_r$ . When, however,  $u$  is such a function that it equals a variable plus a constant, then evidently the constant part is cancelled by subtraction in the expression  $u_v - u_{v-1}$ .

It might then be omitted in the right-hand member, but then we would not have  $u_{n-1} = 0$  if it were  $= 0$  when the constant part was left in. Notice also that  $\theta_{m-1}$  will have the value  $\theta_r$ .

Make

$$\sum_{l_r}^0 \lambda_v = L_r; \sum_{l_r}^0 \gamma_v = G_r; \sum_{l_r}^0 \tau_v = T_r \quad (7)$$

The space over which the summation is taken is shown by the limits. Here it includes the whole span.

$M$  is the moment at the support shown by its subscript.

In Prof. Howe's analysis the  $\varepsilon$  that has the subscript  $l_r$  is the last value but one of  $\varepsilon$ , or in other words, the space between the points whose ordinates are  $\varepsilon_v = l_r$  and  $\varepsilon_v = \varepsilon_1$  is the extreme right end space in the span, and between these points  $\theta$  has the value  $\theta_r$ . A very confusing refinement necessary in his analysis but not in mine.

Divide Prof. Howe's equation (55) by  $\theta_r \theta_{r-1}$ , and we have the following formula. (Some misprints corrected here and elsewhere.)

$$\begin{aligned}
 & M_{r-1} \frac{(l_{r-1} + F'_{r-1})}{\theta_{r-1}} + M_r \left\{ \frac{2(l_r + F_r)}{\theta_r} + \frac{2(l_{r-1} + F'_{r-1})}{\theta_{r-1}} \right\} \\
 & + M_{r+1} \left( \frac{l_r + F'_r}{\theta} \right) = - \left( \frac{l_r - l_{r-1}}{l_{r-1}} + \frac{l_r - l_{r+1}}{l_r} \right) \\
 & - \frac{(\sum P_r l_r^2 (2k_r - 3k_r^2 + k_r^3) + F'_r \sum P_r l_r (1 - k_r) - H_r)}{\theta_r} \\
 & - \frac{(\sum P_{r-1} l_{r-1}^2 (k_{r-1} - k_{r-1}^3) + 2F''_{r-1} \sum P_{r-1} l_{r-1} (1 - k_{r-1}) + H'_{r-1})}{\theta_{r-1}} \quad (8)
 \end{aligned}$$

Prof. Howe considers moments causing compression on upper chord as positive.

He gives definitions as follows (substituting  $\theta$  for  $l$ , which does not change the meaning):

$$\frac{F_r}{\theta_r} = \sum_{v=l_r}^{v=1} \left( \frac{1}{\theta_{v-1}} - \frac{1}{\theta_v} \right) \left( \frac{3 l_r^2 \varepsilon_v - 3 l_r \varepsilon_v^2 + \varepsilon_v^3}{l_r^2} \right) \quad (9)$$

$$\frac{F'_r}{\theta_r} = \sum_{v=l_r}^{v=1} \left( \frac{1}{\theta_{v-1}} - \frac{1}{\theta_v} \right) \left( \frac{3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3}{l_r^2} \right) \quad (10)$$

$$\frac{F''_r}{\theta_r} = \sum_{v=l_r}^{v=1} \left( \frac{1}{\theta_{v-1}} - \frac{1}{\theta_v} \right) \frac{\varepsilon_v^3}{l_r^2} \quad (11)$$

Comparing these equations with equations (2), (3), (4), (6) and (7), it becomes evident that

$$2 \left( \frac{l_r + F_r}{\theta_r} \right) = T_r \quad (12)$$

$$\frac{l_r + F_r'}{\theta_r} = L_r \quad (13)$$

$$\frac{2 (l_r + F_r'')}{\theta_r} = G_r \quad (14)$$

Prof. Howe also gives definitions of  $H_r$  and  $H_r'$ , but in too general a form to be easily apprehended, since it is for a number of weights scattered all over the span.

It is better to consider a single weight only, since the final value of the quantities is the sum of their partial values. A condition that he gives in his analysis is that for each weight  $\varepsilon_v > a_r$ . We might, however, have  $\varepsilon_v = a_r$  to begin with, without changing the result, since in that case  $\varepsilon_v - a_r = 0$ .

The modified definitions which, nevertheless, express his idea, are as follows:

$$\begin{aligned} \frac{H_r}{\theta_r} = & \sum_{v=l_r}^{\varepsilon_v=a_r} \left( \frac{1}{\theta_{v-1}} - \frac{1}{\theta_v} \right) \left( P_r (\varepsilon_v - a_r)^3 \right. \\ & \left. + \frac{3 (l_r - \varepsilon_v)}{l_r} P_r (\varepsilon_v - a_r)^2 \right) \end{aligned} \quad (15)$$

$$\frac{H_r'}{\theta_r} = \sum_{v=l_r}^{\varepsilon_v=a_r} \left( \frac{1}{\theta_{v-1}} - \frac{1}{\theta_v} \right) \left( P_r (\varepsilon_v - a_r)^3 - \frac{3 \varepsilon_v}{l_r} P_r (\varepsilon_v - a_r)^2 \right) \quad (16)$$

Now

$$\begin{aligned} & (\varepsilon_v - a_r)^3 + \frac{3 (l_r - \varepsilon_v)}{l_r} (\varepsilon_v - a_r)^2 \\ = & \frac{3 l_r \varepsilon_v^2 - 6 l_r \varepsilon_v a_r + 3 l_r a_r^2 - 2 \varepsilon_v^3 + 3 a_r \varepsilon_v^2 - a_r^3}{l_r} \\ & - l_r \left( \frac{3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3}{l_r^2} \right) - a_r \left( \frac{3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3}{l_r^2} \right) \\ & - a_r \left( \frac{6 l_r^2 \varepsilon_v - 6 l_r \varepsilon_v^2 + 2 \varepsilon_v^3}{l_r^2} \right) + \frac{3 l_r a_r^2 - a_r^3}{l_r} \end{aligned}$$

Let

$$\frac{a_r}{l_r} \left( (l_r - a_r) \lambda_v - a_r \tau_v \right) = V_r \quad (17)$$

Then by comparison with equation (6)

$$P_r V_r = \frac{P_r l_r^2 (1 - k_r^3)}{\theta_r} + \frac{H_r}{\theta_r} \quad (18)$$

The constant quantity

$$\frac{(3 l_r a_r^2 - a_r^3)}{l_r}$$

disappears by subtraction.

Also

$$\begin{aligned} \frac{(\varepsilon_v - a_r)^3 - 3 \varepsilon_v (\varepsilon_v - a_r)^2}{l_r} &= - \frac{2 \varepsilon_v^3 + 3 a_r \varepsilon_v^2 - a_r^3}{l_r} \\ &= - l_r \left( \frac{2 \varepsilon_v^3}{l_r^2} \right) + a_r \left( \frac{2 \varepsilon_v^3}{l_r^2} \right) - a_r \left( \frac{3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3}{l_r^2} \right) - \frac{a_r^3}{l_r} \end{aligned}$$

Let

$$V_r' = \frac{a_r}{l_r} \left( (l_r - a_r) \gamma_v - a_r \lambda_v \right) \quad (19)$$

Then by comparison with equation (6)

$$P_r V_r' = \frac{P_r l_r^2 (2 - 3 k_r + k_r^3)}{\theta_r} - \frac{H_r'}{\theta_r} \quad (20)$$

The constant quantity

$$\left( - \frac{a_r^3}{l_r} \right)$$

disappearing as before.

Paying attention to the fact that  $(1 - k_r) - (1 - k_r)^3 = 2 k_r - 3 k_r^2 + k_r^3$  and  $2(1 - k_r) - (2 - 3 k_r + k_r^3) = k_r - k_r^3$ , we have

$$\begin{aligned} &P_r \left( (l_r - a_r) L_r - V_r \right) \\ &= \frac{P_r l_r^2 (2 k_r - 3 k_r^2 + k_r^3)}{\theta_r} + \frac{P_r F_r' l_r (1 - k_r) - H_r}{\theta_r} \quad (21) \end{aligned}$$

$$\begin{aligned} &P_r \left( (l_r - a_r) G_r - V_r' \right) \\ &= \frac{P_r l_r^2 (k_r - k_r^3)}{\theta_r} + \frac{2 P_r F_r'' l_r (1 - k_r) + H_r'}{\theta_r} \quad (22) \end{aligned}$$



Put

$$(l_r - a_r) L_r - V_r = \frac{A_r}{P_r} \quad (21a)$$

and

$$(l_r - a_r) G_r - V'_r = \frac{B_r}{P_r} \quad (22a)$$

then the most convenient formulæ, both for analytical and graphical calculation, are

$$(l_r - a_r) \sum \lambda_v + a_r \sum \tau_v = \frac{A_r}{P_r} \quad (21b)$$

$$(l_r - a_r) \sum \gamma_v + a_r \sum \lambda_v = \frac{B_r}{P_r} \quad (22b)$$

These quantities are calculated by interpolating a term as if the part on which  $\theta$  is constant on which the weight stands were divided into two parts. Thus, for instance, a table for  $G_r$  would contain the quantities

$$2 \left( \frac{\varepsilon_v^3 - \varepsilon_{v-1}^3}{\theta_{v-1} l_r^2} \right)$$

Let there be a point whose ordinate is  $\varepsilon_p$  where the value of  $\theta$  changes on the left. Then to the sum of the tabular values from the left support up to this point is to be added

$$2 \left( \frac{a_r^3 - \varepsilon_p^3}{\theta_p l_r^2} \right)$$

and we have the quantity

$$\sum \gamma_v$$

Collecting results and comparing with our equation (8) or Prof. Howe's equation (55), we have for the *theorem of three moments*

$$\begin{aligned} M_{r-1} L_{r-1} + M_r (T_r - G_{r-1}) + M_{r+1} L_r = -(A_r + B_{r-1}) \\ - \left( \frac{h_r - h_{r-1}}{l_{r-1}} + \frac{h_r - h_{r+1}}{l_r} \right) \end{aligned} \quad (23)$$

in which moments are positive causing compression on upper

chord, and the  $h^s$  are positive upwards. To have the moments positive causing tension on upper chord change the sign of the load terms and consider the  $h^s$  as positive downwards.

It is a little more convenient in a graphical calculation to consider the latter to be the case, and is the method pursued by Prof. Merriman, which I shall pursue in deducing this theorem of three moments from the differential equation of the elastic line.

Let  $M_x$  be the moment at a point in the span,  $\mu$  the vertical,  $x$  the horizontal coördinate of the elastic line, and  $\theta_x$  the value of  $\theta$  at this point. The latter corresponds to  $\theta_{v-1}$ .

Then the equation may be written

$$\frac{d^2 \mu}{dx^2} = \frac{6 M_x}{\theta_x} \quad (24)$$

But from Merriman's "Continuous Bridges," we have

$$M_x = M_r - S_r x + P_r (x - a_r)$$

and

$$S_r = \frac{M_r - M_{r+1}}{l_r} + P_r (1 - k_r)$$

whence

$$M_x = M_r \frac{(l_r - x)}{l_r} + \frac{M_{r+1} x}{l_r} - P_r (1 - k_r) x + P_r (x - a_r)$$

It should be remarked that the quantity  $P_r (x - a_r)$  belongs to the expression for the moment at a point on the right-hand side of the weight and that this term or the integrals or other results consequent upon it are to be omitted for points on the left side. This statement is intended to save a useless repetition of formulæ.

Substitute the value of  $M_x$  in equation (24) and integrate there results

$$\frac{d\mu}{dx} = \frac{M_r \left( \frac{6 l_r x - 3 x^2}{l_r} \right) + M_{r+1} \frac{3 x^2}{l_r} - 3 P_r (1 - k_r) x^2 + 3 P (x - a_r)^2}{\theta_x} + C_v \quad (25)$$

Now for

$$x = \varepsilon_v, \frac{d\mu}{dx} = t_v$$

and for

$$x = \varepsilon_{v-1}, \frac{d\mu}{dx} = t_{v-1}$$

Taking these as limits and using the symbol  $\Delta$  as explained in equation (1), we have

$$t_v - t_{v-1} = M_r \Delta \frac{(6 l_r^2 \varepsilon_v - 3 l_r \varepsilon_v^2)}{l_r^2} + M_{r+1} \Delta \frac{3 l_r \varepsilon_v^2}{l_r^2} \\ - P_r (1 - k_r) \Delta 3 \varepsilon_v^2 + P_r \Delta (3 \varepsilon_v^2 - 6 \varepsilon_v a_r)$$

and from equations (2), (3) and (4)

$$t_v - t_{v-1} = M_r (\lambda_v + \tau_v) + M_{r+1} (\lambda_v + \gamma_v) - P_r l_r (1 - k_r) (\lambda_v + \gamma_v) \\ + P_r (l_r (\lambda_v + \gamma_v) - a_r (2 \lambda_v + \gamma_v + \tau_v)) \quad (26)$$

$$l_r (\lambda_v + \gamma_v) - a_r (2 \lambda_v + \gamma_v + \tau_v) = (l_r - a_r) (\lambda_v + \gamma_v) - a_r (\lambda_v + \tau_v)$$

Take the sum of the differences of the tangents for the whole span, remembering that the elastic line consists of a series of shorter lines, and at the points of junction they have the same tangent and ordinate. At the left support the tangent is  $t_r$  and at the right it is  $t_{r+1}$ . All the intermediate tangents will disappear and we get

$$t_{r+1} - t_r = M_r (L_r + T_r) + M_{r+1} (L_r + G_r) \\ - P_r l_r (1 - k_r) (L_r + G_r) - P_r (V_r + V_r') \quad (27)$$

See equations (17) and (19).

Integrating equation (25)

$$\mu = \frac{M_r \left( \frac{3 l_r x^2 - x^3}{l_r} \right) + M_{r+1} \frac{x^3}{l_r} - P_r (1 - k_r) x^3 + P_r (x - a_r)^3}{\theta_x} \\ + C_v x + C_v'$$

For

$$x = \varepsilon_v, \mu = \mu_v, \text{ for } x = \varepsilon_{v-1}, \mu = \mu_{v-1}$$

Also for the constant  $C_v$  we have two equal values.

The first is

$$t_v = \left\{ -M_r \left( \frac{6 l_r \varepsilon_v - 3 \varepsilon_v^2}{l_r} \right) - M_{r+1} \frac{3 \varepsilon_v^2}{l_r} \right. \\ \left. + P_r (1 - k_r) 3 \varepsilon_v^2 - P_r (3 \varepsilon_v^2 - 6 \varepsilon_v a + 3 a_r^2) \right\} \div \theta_x$$

and the second is similar to this except that the subscript of  $\varepsilon$  and  $t$  becomes  $v-1$  instead of  $v$ .

Taking the integral between these limits we have therefore

$$\mu_v - \mu_{v-1} = -M_r \int \left( \frac{3 l_r \varepsilon_v^2 - 2 \varepsilon_v^3}{l_r} \right) - M_{r+1} \int \frac{2 \varepsilon_v^3}{l_r} \\ + P_r (1 - k_r) \int 2 \varepsilon_v^3 + P_r \int \left\{ (\varepsilon_v - a_r)^3 - 3 \varepsilon_v (\varepsilon_v - a_r)^2 \right\} \\ + t_v \varepsilon_v - t_{v-1} \varepsilon_{v-1}$$

and from equations (2), (3) and (4) this becomes

$$\mu_v - \mu_{v-1} = -l_r \left( M_r \lambda_v + M_{r+1} \gamma_v - P_r l_r (1 - k_r) \gamma_v \right. \\ \left. + P_r ((l_r - a_r) \gamma_v - a_r \lambda_v) \right) + t_v \varepsilon_v - t_{v-1} \varepsilon_{v-1}$$

Take the sum of the differences of the vertical ordinates for the whole span and then divide by  $l_r$ . For the left support the vertical ordinate is  $h_r$  and for the right support it is  $h_{r+1}$ , and we have

$$\frac{h_{r+1} - h_r}{l_r} + M_r L_r + M_{r+1} G_r - P_r l_r (1 - k_r) G_r + P_r V'_r = t_{r+1} \quad (28)$$

Reduce the subscripts by unity and we get

$$\frac{h_r - h_{r-1}}{l_{r-1}} + M_{r-1} L_{r-1} + M_r G_{r-1} - P_{r-1} l_{r-1} (1 - k_{r-1}) G_{r-1} \\ + P_{r-1} V'_{r-1} = t_r \quad (29)$$

Subtract equation (28) from equation (27), and then to the remainder add equation (29), and we get

$$\left( \frac{h_r - h_{r+1}}{l_r} + \frac{h_r - h_{r-1}}{l_{r-1}} + M_{r-1} L_{r-1} + M_r (L_r + G_{r-1}) \right. \\ \left. + M_{r+1} L_r - P_r (l_r (1 - k_r) L_r - V_r) \right. \\ \left. - P_{r-1} (l_{r-1} (1 - k_{r-1}) G_{r-1} - V'_{r-1}) \right) = 0 \quad (30)$$

This, as will be seen, is the same as equation (23), except that moments causing tension on upper chord are considered as positive.

It is evident from the preceding analysis that if we had considered the modulus of elasticity as variable, it would not affect the form of the formulæ. It would, however, affect the strains considerably, but  $\theta$  being a product, must of course always be constant over any short portion of the span.

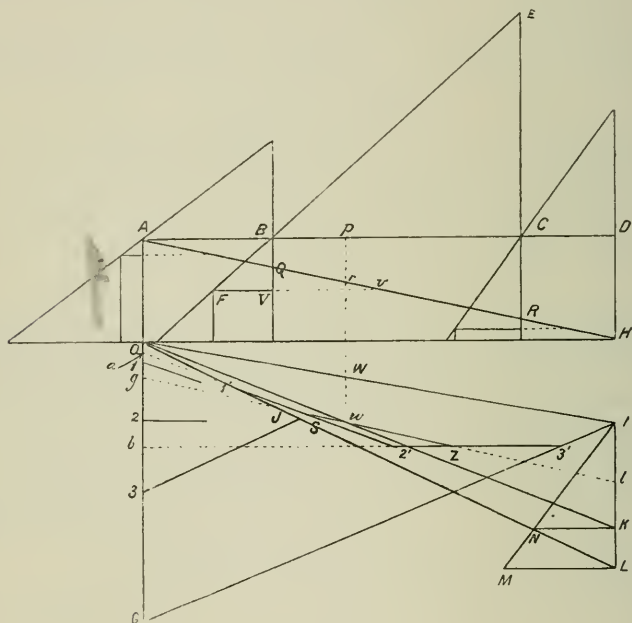


FIG. 1.

The graphical method employed to obtain the necessary quantities  $L_r$ ,  $G_r$ ,  $T_r$ ,  $A_r$  and  $B_r$  remains to be explained. It should be remarked that in practice *Figs. 1* and *2* are constructed on the same paper, but are separated here for the sake of clearness and distinctness in the demonstration.  $A_r$  and  $B_r$  are found for a single position of a load, but it will soon be apparent that these quantities can be found for a single load for an infinite number of positions *from the same diagram*, and this is not possible by any other graphical method that I know of.

I will explain *Fig. 1* by giving the construction first and the proof afterwards.

$\overline{AD}$  represents the span, and the values of  $\theta$  change at  $B$  and  $C$ . Hence  $\overline{AB} = \varepsilon_{v-1}$  and  $\overline{AC} = \varepsilon_v$  for the calculation of  $L_r$ ,  $T_r$  and  $G_r$ . The vertical  $\overline{AO} = \overline{DH}$  is an arbitrary length of line =  $m$ . Complete the parallelogram and draw the diagonal  $\overline{AH}$ . At  $C$  erect the vertical

$$\overline{EC} = \frac{\theta_{v-1} l_r}{6 \theta'}$$

where  $\theta_{v-1}$  is the value of  $\theta$  between  $B$  and  $C$ , and  $\theta'$  is an arbitrary value of  $\theta$ . When, as is customary,  $E$  the modulus of elasticity is assumed to be constant, make

$$\frac{l_r}{6 \theta'} =$$

some easy fraction, such as

$$\frac{1}{10} E' \quad \frac{1}{50} E'$$

or the like. ( $E' = 6 E$ ).

Take

$$\overline{BV} = m \left( \frac{\varepsilon_v + \varepsilon_{v-1}}{2 l_r} \right)$$

which is done by bisecting  $\overline{QR}$  at  $v$  and drawing the horizontal line  $\overline{Vv}$ . This line produced intersects the line  $\overline{BE}$  produced at  $F$ . Consider  $\overline{FV}$  as if it were a vertical force =  $q_v$  applied at the centre of gravity of the area  $\overline{BCRQ}$ . Make the same construction for all the other areas within the triangle  $\overline{ADH}$  and between whose limits the value of  $\theta$  is constant. With these lines as forces construct the force polygon  $O-1-2-3-J$  with an assumed pole distance =  $J$ , and then construct the equilibrium polygon  $O-1'-2'-3'-I$ . Produce the end sides  $\overline{O-1'}$  and  $\overline{3'-I}$  to the verticals through the supports at the ends of the span, then

$$\overline{IL} = \frac{m t'}{J} L_r$$



and

$$\overline{OG} = \frac{m \theta'}{J} G_r$$

The values of  $L_r$  and  $G_r$  are now easily computed, since  $m$ ,  $\theta'$  and  $J$  have been arbitrarily assumed.

The proof is as follows :

$$\overline{FV} = q_v = \frac{\overline{BV} \times \overline{BC}}{\overline{EC}} = \frac{3 m \theta' (\varepsilon_v^2 - \varepsilon_{v-1}^2)}{\theta_{v-1} l_r^2} = \frac{m \theta'}{l_r} (\lambda_v + \gamma_v) \quad (31)$$

Now since

$$\overline{BQ} = \frac{m \varepsilon_{v-1}}{l_r}$$

and

$$\overline{CR} = m \frac{\varepsilon_v}{l_r}$$

then

$$q_v = \frac{6 \theta'}{\theta_{v-1} l_r} (\text{area } BCRQ) \quad (32)$$

The moment of the triangle  $\overline{ACR}$  with reference to the left support is

$$\frac{m \varepsilon_v^2}{2 l_r} \times \frac{2}{3} \varepsilon_v$$

and, therefore, the moment of the area  $\overline{BCRQ}$  is

$$\frac{m l_r}{6} \left( \frac{2 \varepsilon_v^3}{l_r^2} - \frac{2 \varepsilon_{v-1}^3}{l_r^2} \right) = \frac{m l_r \theta_{v-1}}{6} \gamma_v$$

Let  $X_v$  be the distance of the centre of gravity of this area from the left support. Multiply both sides of the preceding equation by

$$\frac{X_v}{J}$$

and for the moment of this area put its value as found above, and we have

$$\frac{q_v X_v}{J} = \frac{m \theta'}{J} \gamma_v \quad (33)$$

This is represented by the length of a line on the vertical through the left end of the span, and the method is a

familiar one to those who use the equilibrium polygon, but it would be perhaps better to explain this here. In *Fig. 1*, in the force polygon,  $\overline{1 \cdot 2} = \overline{F V} = q_v$ . Prolong the sides of the equilibrium polygon  $\overline{1' \cdot 2'}$  to  $a$  and  $\overline{2' \cdot 3'}$  to  $b$ , the latter being points on the vertical through the left support. Then  $\overline{ab} : \overline{1-2} :: X_v : J$  or

$$\overline{ab} = \frac{q_v X_v}{J}$$

Hence

$$\overline{OG} = \Sigma \frac{q_v X_v}{J} = \frac{m \theta' G_r}{J}$$

The moment of the triangle  $ACR$  with reference to the right support is

$$\frac{m \varepsilon_v^2}{2 l_r} \left( l_r - \frac{2}{3} \varepsilon_v \right)$$

whence we easily deduce as before that the moment of the area  $B \overline{CRQ}$  with reference to same support is

$$\frac{m l_r \theta_{v-1}}{6} \lambda_v$$

Multiply both sides of equation (32) by

$$l_r - \frac{X_v}{J}$$

we obtain in a similar manner

$$\frac{q_v (l_r - \frac{X_v}{J})}{J} = \frac{m \theta'}{J} \lambda_v \quad (34)$$

Hence

$$\overline{IL} = \Sigma \frac{q_v (l_r - \frac{X_v}{J})}{J} = \frac{m \theta'}{J} L_r$$

The next is the calculation of  $B_r$  in *Fig. 1*. The weight is located at  $p$ , so that  $A p = a_r$  and  $\theta$  is constant (as was said before) between  $B$  and  $C$ . Suppose the area  $B \overline{CRQ}$  divided into two areas by the vertical through  $p$ . The centre of gravity of the partial area  $B \overline{p r Q}$  is in a vertical through  $s$ , and that of the area  $p \overline{C R r}$  is in a vertical

through  $s$ . Now, since the moment of the whole area is the resultant of those of the partial areas, it is evident that  $\overline{O \cdot I' \cdot s \cdot s' \cdot I}$  is the equilibrium polygon we would have had if we had originally divided the area and made the graphical construction previously shown.

Draw  $\overline{s \cdot s}$  and produce it to the verticals through the supports,  $g$  being the left intersection and  $l$  the right. Then by previous analysis

$$\overline{Og} = \frac{m}{J} \frac{\theta'}{a_r} \sum \gamma_v$$

$$Il = \frac{m}{J} \frac{\theta'}{l_r} \sum \lambda_v$$

and by similar triangles

$$\overline{lK} = \overline{Og} \frac{l_r - a_r}{a_r} = \frac{m}{J} \frac{\theta'}{a_r} \left( \frac{l_r - a_r}{a_r} \right) \sum \gamma_v$$

and, therefore,

$$\begin{aligned} \overline{lK} &= \frac{(l_r - a_r) \sum \gamma_v + a_r \sum \lambda_v}{a_r} \times \frac{m}{J} \frac{\theta'}{l_r} = \frac{m}{J} \frac{\theta'}{P_r} \cdot \frac{B_r}{P_r} \\ W_o &= \overline{lK} \frac{a_r}{l_r} = \frac{m}{J} \frac{\theta'}{l_r} \cdot \frac{B_r}{P_r} \quad (\text{Fig. I.}) \end{aligned}$$

The value of  $B_r$  is now known, but it is sometimes more convenient to calculate the moments at the supports directly by a graphical construction, using the "fixed points." In that case we will want

$$\frac{B_r}{L_r l_r}$$

Now, since

$$\overline{lL} = \frac{m}{J} \frac{\theta'}{L_r} L_r$$

draw

$$LM = \frac{P_r a_r}{l_r}$$



The construction will be plainer when the proof is given that

$$\bar{O} L = \frac{m \theta'}{J} L_r,$$

and

$$U T = \frac{m \theta'}{J} T_r,$$

$$\bar{F} n = \bar{V} d = m \left( 1 - \frac{\varepsilon_v + \varepsilon_{v-1}}{2 l_r} \right)$$

whence

$$\begin{aligned} \bar{f} n = q_v' &= \frac{\bar{F} n \times BC}{E C} = \frac{m \theta'}{\theta_{v-1} l_r} \left( 6 (\varepsilon_v - \varepsilon_{v-1}) - \frac{3 (\varepsilon_v^2 - \varepsilon_{v-1}^2)}{l_r} \right) \\ &= \frac{m \theta'}{l_r} (\lambda_v + \tau_v) \end{aligned}$$

also

$$\bar{F} n = \frac{\bar{Q} d + R \varepsilon}{2}$$

whence

$$\bar{f} n = q_v' = \frac{6 \theta'}{\theta_{v-1} l_r} (\text{area } \bar{Q} R \varepsilon d) \quad (35)$$

The moment of the triangle  $\bar{Q} H d$ , with reference to the left support is

$$\begin{aligned} & \frac{m (l_r - \varepsilon_{v-1})^2}{2 l_r} \left( l_r - \frac{2}{3} (l_r - \varepsilon_{v-1}) \right) \\ &= \frac{m}{6 l_r} (3 l_r^3 - 6 l_r^2 \varepsilon_{v-1} + 3 l_r \varepsilon_{v-1}^2 - 2 l_r^3 - 6 l_r^2 \varepsilon_{v-1} - 6 l_r \varepsilon_{v-1}^2 \\ & \quad + 2 \varepsilon_{v-1}^3) = \frac{m}{6 l_r} (l_r^3 - 3 l_r \varepsilon_{v-1}^2 + 2 \varepsilon_{v-1}^3). \end{aligned}$$

Hence the moment of the area  $Q R \varepsilon d$ , with reference to the same support is

$$= \frac{m l_r \theta_{v-1} \lambda_v}{6}$$

Let  $N_v'$  be the distance of the centre of gravity of this area from the left support. Multiply both sides of equation (35) by

$$\frac{N_v'}{J}$$



and we have

$$\frac{q_v' X_v'}{J} = \frac{m \theta'}{J} \lambda_v \quad (36)$$

Hence

$$\overline{O L} = \sum \frac{q_v' X_v'}{J} = \frac{m \theta' L_r}{J}$$

Of course this value of  $L_r$  ought to be the same as that obtained from *Fig. 1*. Owing to unpreventable errors it will seldom be exactly the same but any discrepancy shows that there is error and if too great the construction must be repeated.

The moment of the triangle  $\overline{Q H d}$  with reference to right support is

$$\frac{m (l_r - \varepsilon_{v-1})^2}{2 l_r} \times \frac{2}{3} (l_r - \varepsilon_{v-1})$$

whence we deduce moment of area  $\overline{Q R \varepsilon d}$  with reference to this support is

$$\frac{m l_r \theta_{v-1} \tau_v}{6}$$

Multiply both sides of equation (35) by

$$\frac{l_r - X_v'}{J}$$

and we have

$$q_v' \frac{(l_r - X_v')}{J} = \frac{m \theta'}{J} \tau_v \quad (37)$$

Hence

$$\overline{U T} = \sum \frac{q_v' (l_r - X_v')}{J} = \frac{m \theta'}{J} T_r$$

The value of  $A_r$  is found in a way much similar to the way  $B_r$  is found in *Fig. 1*.  $s$  is in the vertical through the centre of gravity of the partial area  $\overline{Q r p' d}$ , and  $z$  in that of the area  $r R \varepsilon p'$ . Hence  $O-1'-s-s'-3'-U$  is the equilibrium polygon we would have obtained had we divided the area  $\overline{Q R \varepsilon d}$  in the first place.

$$\overline{O l} = \frac{m \theta'}{J} \sum \frac{\lambda_v}{a_r}$$

$$\overline{U} t = \frac{m \theta'}{J} \frac{a_r}{l_r} \tau_v$$

and by similar triangles

$$\overline{I} K = \overline{U} t \frac{a_r}{l_r - a_r} = \frac{m \theta'}{J} \frac{a_r}{l_r - a_r} \frac{a_r}{l_r} \tau_v$$

whence

$$\begin{aligned} \overline{O} K &= \frac{(l_r - a_r) \frac{a_r}{l_r} \lambda_v + a_r \frac{a_r}{l_r} \tau_v}{\frac{a_r}{l_r - a_r}} \times \frac{m \theta'}{J} \\ &= \frac{m \theta'}{J} \frac{A_r}{P_r (l_r - a_r)} \end{aligned}$$

$$\overline{W} \omega \quad (\text{Fig. 2}) = \overline{O} K \frac{l_r - a_r}{l_r} = \frac{m \theta'}{J l_r} \frac{A_r}{P_r}$$

Let

$$\overline{L} M = \frac{P_r (l_r - a_r)}{l_r}$$

then

$$\overline{N} K = \frac{\overline{L} M \cdot \overline{O} K}{\overline{O} L} = \frac{A_r}{L_r l_r}$$

This is a quantity needed if the moments at the supports are directly graphically calculated by means of the "fixed points."

The length of the ordinate  $\overline{W} \omega$  in both figures is independent of the intensity of the load. Hence if the load were of an elementary intensity  $= p \, dx$  (where  $p$  is the weight per lineal unit and  $dx$  an elementary length) then the area formed by  $\overline{W} \omega$  moving horizontally over the span would be proportional to  $A_r$  or  $B_r$  respectively for a uniform load over the portion moved over. It should be noticed however that the point  $\omega$  moves in a curve that is tangent to the original equilibrium polygon at the point where the value of  $\theta$  changes.

This completes the demonstration. It may be said to be

somewhat tedious but the same cannot be said of the graphical method. It is theoretically exact.

By it one may obtain results in a few hours that by the analytical or any other graphical method it might take weeks to accomplish and the author has found that the error need not exceed one or two per cent. if only ordinary care is used. The process throughout is as nearly mechanical as possible, thus not only preventing the danger of gross error but also getting rid of the burden of mental labor resulting from the algebraic numerical computations necessary in other methods.

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SOME FACTS RELATING TO THE ARTICLE ON THE  
FORCE OF IMPACT OF WAVES AND THE STA-  
BILITY OF THE SUPERSTRUCTURE OF  
BREAKWATERS.

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BY L. D'AURIA.

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In the *Journal of the Franklin Institute*, for November, 1890, the writer, in his article "On the Force of Impact of Waves and the Stability of the Superstructure of Breakwaters," for the first time enunciated the following general principle: "The amount of energy which is necessary to cause the propagation of a wave is equal to the energy which such wave would require if it had to be moved *en masse* with its velocity of propagation upon a frictionless level surface" (p. 373.) This energy, when computed per linear foot of wave crest, is approximately expressed by

$$E = \frac{s k \gamma v^2}{4 g}$$

in which  $s$  is the wave length;  $k$  its height measured from crest to trough;  $\gamma$  the weight of one cubic foot of water; and  $g$  the acceleration of gravity.

When a wave comes in contact with a breakwater the energy  $E$  is discharged against such structure in the interval of time which the wave requires to propagate its own length, so that, denoting by  $f$  the mean force of impact of

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the wave against every foot of breakwater during such interval of time, we find

$$f = \frac{k \gamma v^2}{4g}$$

According to observations the force of impact of waves against breakwaters is manifested only on that portion of the superstructure extending from crest to trough, so that, such force, at the time when the trough comes in contact with the breakwater, it must be zero and must gradually increase toward a maximum. Hence, the maximum force of impact per linear foot of superstructure can be expressed approximately by twice the mean, or by

$$F = \frac{\gamma k v^2}{2g}$$

(See equation (1), *Journal Franklin Institute*, p. 374, November, 1890.)

The mean force of impact per square foot, deduced from this equation, is

$$p = \frac{\gamma v^2}{2g}$$

and since the force of impact per square foot is found to vary from zero to a maximum, this maximum can be expressed approximately by twice the mean, or by

$$P = \frac{\gamma v^2}{g} \quad (1)$$

This ought to accord with the maximum force of impact which can be observed by means of marine dynamometers placed against the superstructure of breakwaters. This has been proved to be a fact by the editor of *Engineering News*, in his editorial article on "Wave Pressure," published September 6, 1890, two months before the writer's article on the force of impact of waves, etc., appeared in the *Journal of the Franklin Institute*. This article, however, is a report the writer made to Col. Henry M. Robert, Corps of Engineers, U.S.A., while in the U. S. Engineer Office of Philadelphia, and was filed January 24, 1890. It was begun sometime in 1889 when the project to close the gap of the

Delaware Breakwater was being studied in that office. At that time the Assistant Engineer, Mr. L. Y. Schermerhorn, consulted the best works on the subject, but was unable to find any formula by means of which the maximum force of impact of a given wave against every linear foot of the superstructure of a breakwater could be ascertained, and the proper dimensions to be given to such superstructure determined. The want for such formulæ has always been felt by marine engineers, and it is no mean achievement for any one who succeeds in making such contribution to the dynamics of waves. The question now is how the editor of *Engineering News* reached the conclusion given in formula (1) of this paper, without the least knowledge of the energy of waves, as is shown by the manner in which he has handled the subject. Had there been no proposal for building the Diamond Shoal Lighthouse perhaps no one would have discovered that the editor of *Engineering News* possessed such valuable information regarding the wave pressure, as he calls it, for it was on this occasion, and immediately after the contract for that lighthouse had been awarded, that he presented himself as an authority in matters relating to wave pressure. This occurred on September 6, 1890, in his editorial already mentioned, in which, speaking of the bidders and the views they took in regard to wave action against the proposed lighthouse, he says: "There was evidently a decided contrast of views in regard to this question among the bidders on the structure, all of whom were engineers or contractors of high reputation."

Then, after reviewing the project of one of the bidders, he asks the question, "Are those allowances sufficient for a case of this kind?" To determine this he proceeds, saying, "Let us first run over certain general facts and theories," which he does, and accordingly reaches the conclusion, by Rankine's formula, that a wave 600 feet long, forty-three feet high, and moving at a rate of fifty-four feet per second, would probably at sea exert a pressure per square foot not exceeding 300 pounds. But he says: "The preceding laws hold only when the water is about as deep as the length of a wave or several hundred feet. Any person accustomed to surf-bath-



ing does not need to be told that the whole upper surface of the water moves forward with great speed on the crests and with hardly any in the troughs; while there is a constant undertow setting out from shore."

It seems that neither the bidders for the Diamond Shoal Lighthouse (all of whom are engineers or contractors of high reputation) nor any of the investigators or able marine engineers who have studied the subject in question, ever enjoyed the pleasure of a surf-bath, for otherwise they would have thought themselves warranted in assuming, as the editor of *Engineering News* assumes, without any other support, that "the surface of the sea in a harbor or on a bank moves forward with a velocity equal to that of the wave," and would have arrived at the "interesting results" found by him on such assumption. But let us see how he reaches these results. He quotes Rankine's *Civ. Eng.*, p. 754, in which this celebrated author says: "In water that is very shallow, compared with the length of the wave, the velocity is nearly independent of the length, and is nearly equal to that acquired by a heavy body in falling through half the depth of the water added to three-fourths of the height of a wave." The editor of *Engineering News*, however, instead of "very shallow compared with the length," substitutes, on his own authority, "when the depth of water is only a few times the height," and then proceeds to find the velocity of the waves observed at Wick, Scotland, which waves were forty feet high, and moved in water a trifle over sixty feet deep. Hence, he finds for such waves a velocity of sixty-two feet per second. Armed with this velocity, he considers the wave itself as a jet impinging upon the breakwater with such velocity, applies the formula for the impact of a jet of water against an unlimited plane surface, and comes out triumphantly with a wave pressure of 3.7 tons per square foot. Here he remembers for an instant that according to Rankine's formula, he had found that in the open sea the maximum pressure per square foot exerted by the greatest-observed wave would not exceed 300 pounds, is staggered for a moment, and then remarks: "This result seems absurd as compared with that just deduced for the pres-

sure of waves in mid-ocean, but the absurdity is only apparent."

The reason given for this is that a maximum pressure observed by marine dynamometers is very nearly the one he has found by \* \* \* experience in surf-bathing, in the absence of any theory. He, therefore, concludes: "Hence it will be seen that the assumption of a velocity of the particles of water equal to that of the wave, although not in accordance with theory, still leads to results agreeing with experiments." On the contrary, we have shown at the head of this paper that our theory leads exactly to the same results which are obtained by experiments. The principle upon which this theory is based has been proved to be correct in the article, "A New Theory of the Propagation of Waves in Liquids," which the writer has published in the *Journal of the Franklin Institute* for December, 1890. In this article, by the application of the same principle above-mentioned, a general formula for the velocity of propagation of any wave has been obtained, which, when applied to various cases, has shown the most remarkable agreement between practice and theory. Take, for example, the case of the tidal wave in which it is known that the horizontal displacement of the liquid particles from surface to bottom is constant, we have found that the velocity of propagation of such wave would be equal to that which a heavy body would acquire in falling through half the depth, a formula well known to those familiar with the subject, and which the editor of *Engineering News* evidently ignored. In fact, had he been acquainted with it, he would have understood Rankine differently and would have noticed that the generalization which this illustrious scientist had in view when he assumed the depth to be very shallow compared with the length of the wave, was referred to tidal waves, not to waves forty feet high and probably 500 feet long, moving in water sixty feet deep. In a wave of the class which was intended by Rankine and to which his generalization could be applied, the liquid particles are supposed to possess the same velocity of translation at the same instant and at all depths in the same cross-section, and the vertical motion must be supposed inconsider-

able in comparison with the horizontal motion. The tidal waves which are observed in tidal rivers are nearly waves of this kind, and the pressure which such waves exert upon a plane surface at right angle to their motion is almost insignificant compared with that due to their velocity of propagation.

It seems strange that the editor of *Engineering News*, while in a state of such ignorance about the whole subject of waves (with the exception of surf-bathing), should undertake to catechise "engineers and contractors of high reputation" on this very subject.

The most remarkable thing, however, about the incident, is that the editor of *Engineering News*, in the issue of November 15th, while engaged in the discussion of wave pressure with a correspondent, did not notice the writer's article on the force of impact of waves, etc., published in the *Journal of the Franklin Institute*, for November, 1890, though he noticed and reprinted from this *Journal* an article by Theo. N. Ely, on "Steam Heating of Passenger Trains," which ends on the same page where the article "On the Force of Impact of Waves," etc., begins. (See p. 373, *Journal of the Franklin Institute*, November, 1890.)

That the editor of *Engineering News* could fail to notice this article which, at that time, ought to have concerned him most, is impossible, and he did not even protest against the writer's arrogation of originality. Was it magnanimity, or was the editor of *Engineering News* aware of the fact that the writer's article had been filed in the United States Engineer Office of Philadelphia, January 24, 1890?

Indeed, this last alternative is not improbable, since the editor of *Engineering News* is not a stranger at that office. In fact, he is an old friend and schoolmate of the assistant engineer, with whom he often corresponds on engineering matters, and it may be that accidentally the conclusions reached by the writer in the above paper were propagated, so to speak, from Philadelphia to New York, which would solve the enigma without adding another word in the way of explanation.

PROCEEDINGS  
OF THE  
CHEMICAL SECTION,  
OF THE  
FRANKLIN INSTITUTE.

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[*Stated meeting, held at the institute, Tuesday, December 16, 1890.*]

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, December 16, 1890.

Mr. T. C. Palmer, president, in the chair.

Members present : Prof. E. F. Smith, Prof. L. B. Hall, Dr. Wm. H. Wahl, Mr. Lee K. Frankel, Mr. H. Pemberton, Jr., Mr. Reuben Haines, Dr. S. C. Hooker, Prof. Henry Trimble, and several visitors.

Dr. Wahl, as chairman of the committee to prepare a circular to be addressed to the chemists of the country, reported progress, and the committee was continued. The secretary read his annual report to the section, which was accepted.

The election of officers followed, and, on motion, it was decided that the secretary cast the vote of the section for the ticket nominated at the November meeting. Dr. Wahl was escorted to the chair by the retiring president, and, on taking his seat, made a brief address appropriate to the occasion.

On motion of Dr. Hall, it was voted that the officers, president, secretary and treasurer, constitute three of the seven members of the committee on admissions.

The following gentlemen were then nominated and elected to fill the remaining vacancies in the committee, viz : Mr. H. Pemberton, Jr., Dr. S. E. Hooker, Prof. E. F. Smith, Dr. L. B. Hall.

The president appointed Messrs. Haines and Frankel to act with the secretary as members of the finance committee.

Dr. Wahl referred to the approaching convention of chemists, to be held shortly in this city, stating what had been done by the local committee to arrange for the event, and expressed the wish that the members of the section would second the efforts of the local committee to make the visitors welcome.

Dr. Greene exhibited specimens of the alloys of sodium and lead, discussed in the paper by himself and Dr. Wahl at the last meeting. They were examined with much interest.

Adjourned.

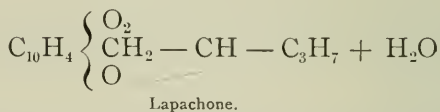
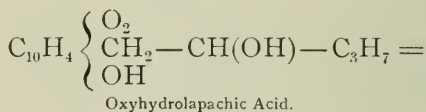
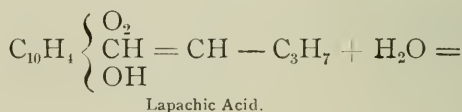
WM. C. DAY, *Secretary*.

## ON SOME DERIVATIVES OF LAPACHIC ACID (I).

By SAMUEL C. HOOKER and WM. H. GREENE.

[Read before the Chemical Section, March 18, 1890.]

In a preliminary paper\* published by the authors, it was shown that the conversion of lapachic acid into lapachone,† by the action of strong mineral acids, probably occurs in two stages, as indicated in the following equations:



In support of this view it was stated that lapachone can be readily converted into oxyhydrolapachic acid, and that this acid, under the influence of mineral acids, again readily passes into its anhydride lapachone.

The details of these experiments, which have not yet been printed, will be published in this paper.

## CONVERSION OF LAPACHONE INTO OXYHYDROLAPACHIC ACID.

The action of aqueous potash on lapachone has been studied by Paterno,‡ who writes as follows: "In aqueous potash of medium concentration lapachone does not dissolve in the cold; on heating it passes into a fine purple-red

\* This *Journal*, **128**, 142.† *Gazz. chim. Ital.*, **12**, 372.‡ *Gazz. chim. Ital.*, **12**, 372.

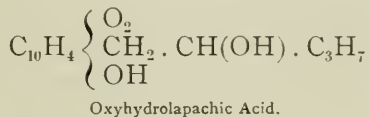
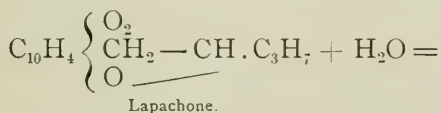


solution which, filtered while hot, deposits on cooling beautiful orange needles of silky lustre, recognized by their fusing point ( $154^{\circ}$ – $155^{\circ}$ ) to be lapachone. On the addition of hydrochloric acid the alkaline filtrate yields a precipitate, which, purified by recrystallization, was similarly found to be lapachone."

This statement is misleading. The facts are these: Lapachone dissolves with some difficulty in hot aqueous potash, but in so doing undergoes a change. A new compound, an acid, is formed, and this, and not lapachone, exists in the solution obtained. If hydrochloric acid, in very slight excess, be added to the cold alkaline solution, the new compound separates as a yellow oil, which gradually assumes a crystalline form. If, however, a larger quantity of hydrochloric acid be employed, the color of the turbid solution is seen gradually to change. The bright yellow gives place to orange, and finally red crystals of lapachone may be observed floating in the liquid.

The new compound has, in fact, been reconverted, by the excess of hydrochloric acid employed, into lapachone. The experiment has been repeated a number of times, but the authors have not once observed the separation of crystals of lapachone from the alkaline solution as described by Paterno.

The action of potash on lapachone is shown in the following equation:



In order to prepare oxyhydrolapachic acid, eight grams of lapachone, four grams of caustic potash and 150 cc. of water are heated together. If the crystals of lapachone be large, they should be first powdered. As the lapachone is

dissolved, the color of the solution becomes very intense, being similar to that of the salts of lapachic acid.

After boiling several minutes, the solution is filtered to remove any crystals which may have escaped the action of the potash, and an excess of acetic acid is then added. A yellow oil is immediately precipitated, which collects in the bottom of beaker, and then appears considerably darker than when first seen in a fine state of division. In the course of an hour or so, it solidifies to a yellow crystalline mass which, after some hours, may be separated and washed well with water. The acid, as thus obtained, though slightly colored at the surface, is in a very pure condition and can be rendered absolutely so by crystallization once or twice from alcohol, in which it dissolves very readily and from which it separates slowly in monosymmetric crystals. The yield is theoretical; eight grams of lapachone gave 8.4 grams of the crude acid; that required by theory being 8.59 grams.

The following figures were obtained on analysis:

(I)	1.965 gram gave	1.4970 CO <sub>2</sub>	and	—	H <sub>2</sub> O
(II)	1.4932	" "	1.2510 CO <sub>2</sub>	"	1.2743 H <sub>2</sub> O
(III)	1.2093	" "	1.5306 CO <sub>2</sub>	"	1.1160 H <sub>2</sub> O

	I.	Found. II.	III.	Calculated for C <sub>15</sub> H <sub>10</sub> O <sub>4</sub> .
C, . . . . .	68.96	69.18	69.13	69.23
H, . . . . .	lost	6.18	6.15	6.15

Oxyhydrolapachic acid melts at 125°. It is readily soluble in most of the ordinary solvents, from which it crystallizes after standing some time; if, however, the solvent be allowed to evaporate rapidly, the acid is left as a yellow oil. Under ordinary conditions it is a perfectly stable body, which can be crystallized from acetic acid unchanged; but in contact with mineral acids, even when dilute, it is readily converted into its anhydride lapachone, which was identified by its fusing point and other properties, and by analysis.

The barium salt is extremely characteristic, separating from a claret-colored solution in bright *orange*, silk-like needles, grouped together in wavy tufts. In order to prepare it, the acid was dissolved in a solution of baric hydrate,

from which the excess of barium was precipitated at the boiling temperature by means of carbon dioxide. The solution was then concentrated to the crystallizing point. As the evaporation proceeds, a film of the salt, in an amorphous condition, and of the *same* color as the solution, forms at the edge of the liquid, on the evaporating basin. The salt is much more soluble in hot than in cold water. After recrystallization, the orange needles gave the following figures on analysis:

(I)	·2152	gram substance	gave	·0744	BaSO <sub>4</sub> .
(II)	·2883	"	"	·0987	"
(III)	·2478	"	"	·0854	"

	<i>Found.</i>		<i>Calculated for</i>		
	<i>I.</i>	<i>II.</i>	<i>III.</i>	(C <sub>15</sub> H <sub>15</sub> O <sub>4</sub> ) <sub>2</sub> Ba, H <sub>2</sub> O.	(C <sub>15</sub> H <sub>15</sub> O <sub>4</sub> ) <sub>2</sub> Ba.
Ba (per cent.), .	20·32	20·12	20·26	20·35	20·91

The above figures were obtained from different preparations, dried over sulphuric acid, and also at 110°.

The salt evidently contains one molecule of water of crystallization, although the attempts to determine this by loss were unsuccessful. Heated to 110° for several hours, no change in weight occurred, and decomposition commenced at somewhat higher temperatures. It was, however, observed that the orange crystals, when rubbed, became dark magenta-red, and analysis proved the altered salt, dried at 105°–110°, to be anhydrous. Under the microscope, it showed no definite structure, and when moistened with water it became immediately orange, at the same time swelling up and then passing into solution. On evaporation, the characteristic orange needles of the salt, as above described, were again obtained.

The anhydrous salt was prepared for analysis by thoroughly grinding the orange crystals, in small quantities at a time, in an agate mortar. The conversion was not easily accomplished, and it seemed as if a resinous substance were being dealt with. The ground substance was heated to 110° until constant in weight.

(I)	·3368	gram substance	gave	·1182	BaSO <sub>4</sub> .
(II)	·2834	"	"	·1002	"

	<i>Found.</i>		<i>Calculated for</i>
	<i>I.</i>	<i>II.</i>	(C <sub>15</sub> H <sub>15</sub> O <sub>4</sub> ) <sub>2</sub> Ba.
Ba, . . . . .	20·63 p.c.	20·78 p.c.	20·91 p.c.

The two determinations were made with different preparations.

The calcium salt is entirely different from the barium salt just described. It separates from its solution on evaporation in small dark-red crystals, and is only very sparingly soluble in both hot and cold water. After drying over sulphuric acid, it did not lose in weight when exposed for over an hour to a temperature of  $105^{\circ}$ – $110^{\circ}$ . Analysis shows it to be anhydrous.

(I) .3113 gram gave .0808  $\text{CaSO}_4$   
 (II) .2594 " " .0635 "

	I.	Found.	II.	Calculated for ( $\text{C}_{15}\text{H}_{15}\text{O}_4$ ) $_2\text{Ca}$ .
Ca., . . . . .	7.63 p.c.		7.19 p.c.	7.16 p.c.

The preparations analyzed were different. (I) was obtained by adding the theoretical quantity of calcium chloride to the slightly ammoniacal solution of the acid; on expelling the excess of ammonia by heat, the salt separated. (II) was prepared as above described for orange variety of the barium salt.

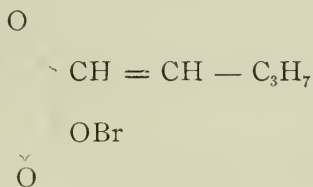
The silver salt was obtained by precipitating a concentrated neutral solution of the ammonium salt with the calculated quantity of silver nitrate. It separates in a semi-resinous condition, becoming granular after some time. It is decidedly soluble in water, also in dilute alcohol, from which it separates, on spontaneous evaporation, in small dark-red needles. The figures it gave on analysis lie between those required for  $\text{C}_{15}\text{H}_{15}\text{O}_4\text{Ag}$  and  $\text{C}_{15}\text{H}_{15}\text{O}_4\text{Ag} + \text{H}_2\text{O}$ . It is probable, therefore, that the compound was not obtained in a pure condition.

## ON SOME DERIVATIVES OF LAPACHIC ACID (II).

BY SAMUEL C. HOOKER.

[Read before the Chemical Section, Franklin Institute, May 20, 1890.]

Through the action of bromine on lapachic acid in acetic acid solution, Paterno\* obtained a compound  $C_{15}H_{13}BrO_3$ , which he regarded as bromlapachic acid, and in which he believed the bromine to have replaced the hydroxylic hydrogen of lapachic acid. He, therefore, assigned to it the formula†



In selecting this formula, Paterno was guided by the following reactions:

(1) Lapachic acid dissolves readily in alkalies in the cold, but bromlapachic acid does not.

(2) The monacetyl derivative of lapachic acid yields on treatment with bromine bromlapachic acid.

(3) On oxidation with nitric acid phthalic acid, not bromphthalic acid, is formed.

(4) Unlike lapachic acid, bromlapachic acid undergoes no changes in contact with concentrated sulphuric acid.

In view of the light thrown on the formation of lapachone from lapachic acid by the author in conjunction with Dr. Wm. H. Greene,‡ it seems probable that the action of bromine on lapachic acid takes place as indicated in the

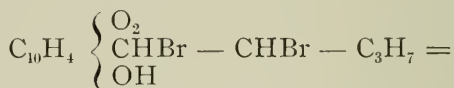
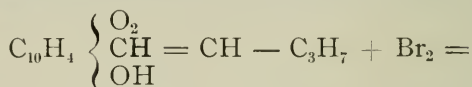
\* *Gazz. chim. Ital.*, **12**, 353.

† It still remains an open question whether lapachic acid is derived from  $\alpha$ - or  $\beta$ -naphthaquinone. Paterno is occupied in solving this question. See also preceding paper.

‡ This *Journal*, **128**, 147.

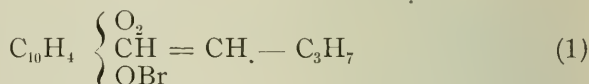


following equations, and that the resulting compound is bromlapachone, and not bromlapachic acid.



This view was advanced by Hooker and Greene\* about a year ago, without, however, being supported by direct experimental evidence. It was afterwards discussed by Paterno and Minunni† who, although admitting it had some strong points in its favor, did not appear to regard it on the whole as probable.

In order to distinguish between the formulæ



and



the action of potash on the so-called bromlapachic acid has been studied, as it seemed probable that, if its constitution is correctly represented by the formula (1), lapachic acid would be again formed by the removal of the bromine. If, on the other hand, the compound is a derivative of lapachone it should behave similarly to lapachone (see preceding paper) in contact with potash.

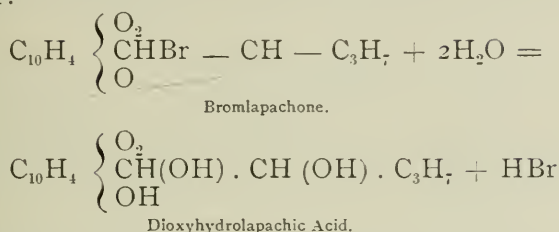
The results obtained prove unmistakably that Paterno's so-called monobromlapachic acid is in reality monobromlapachone.

The action of dilute potash upon the compound at

\* This *Journal*, **128**, 147.

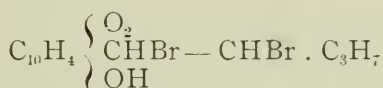
† *Gazz. chim. Ital.*, **19**, 623.

the boiling temperature is represented in the following equation:



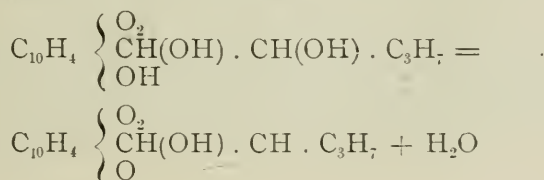
If bromlapachone had the formula which Paterno assigns to it as bromlapachic acid, it should form an addition compound with bromine. The author has allowed it to remain for several days in contact with the theoretical quantity of bromine in chloroform solution, but has not observed the formation of any addition compound. Bromlapachone was recovered unchanged.

In order to remove all doubts as to the mechanism of the reaction resulting in the formation of bromlapachone, the study of the action of bromine on lapachic acid in chloroform solution has been undertaken in the hope of isolating the intermediate product



In the course of this investigation no less than five compounds have been obtained by the action of one molecule of bromine on one molecule of lapachic acid. The study of these interesting bodies has had to be temporarily suspended, owing to dearth of material, but it is hoped that the work will be soon resumed and the results soon ready for publication.

By the action of dilute mineral acids upon dioxyhydrolapachic acid it is converted with great ease into its anhydride, oxylapachone, as follows:



Further, just as oxyhydrolapachic acid results from the action of alkalies on lapachone, so dioxyhydrolapachic acid results from their action on oxylapachone.

#### BROMLAPACHONE.

In the course of some experiments, already referred to, it was observed that, under certain conditions, bromlapachone (Paterno's monobromlapachic acid) could be readily obtained by the action of bromine on lapachic acid in chloroform solution. As the yield is much larger and the substance purer than when prepared in acetic acid solution, as described by Paterno, this method of preparation was employed. It will be described in detail when the compounds arising from the action of bromine on lapachic acid in chloroform solution are discussed.

The bromlapachone was first carefully compared with that prepared as directed by Paterno, and in all particulars was found to be identical. A bromine estimation was made, with the following result:

2588 grm. substance gave 1502 AgBr.

	Found.	Calculated for $C_{15}H_{13}BrO_3$ .
Br, . . . . .	24 69	24'92

Bromlapachone has been observed by Paterno to separate from its solution in alcohol in orange-red crystalline plates. While able to confirm this observation, the author has also obtained it, when in a pure form, crystallized in tufts of fine needles. The conditions under which both varieties of crystals are formed may be stated generally as follows:

From slightly impure alcoholic solutions bromlapachone separates in plates. The same is true whether the solution be allowed to remain at perfect rest, or be disturbed while crystallization is proceeding.

From pure concentrated solutions bromlapachone can be readily obtained in plates by keeping the solution slightly in motion while the compound is crystallizing out.

If the compound, crystallized in plates and still in contact with its saturated mother liquor, be allowed to stand one or more weeks, the plates will be gradually replaced by tufts of needles. This change is believed to occur only when the

compound is in a very pure condition. The observation was first accidentally made, and the experiment was then repeated several times.

Pure solutions protected from dust and allowed to stand absolutely at rest frequently deposit the compound crystallized entirely in tufts of needles.

By observing only partially the above conditions both varieties of crystals may often be obtained in some solution.

The above remarks apply to all solutions of bromlapachone, whether obtained from needles or plates.

The melting points of the two varieties of crystals differ only very slightly, if at all. That of the plates was found to be  $138^{\circ}\cdot 5$ ; that of the needles,  $138^{\circ}$ .

A combustion of the needles gave the following results:

·2399 grm. substance gave ·4932  $\text{CO}_2$  and ·0870  $\text{H}_2\text{O}$ .

	Found.	Calculated for $\text{C}_{15}\text{H}_{13}\text{BrO}_3$
C, . . . . .	56·06	56·07
H, . . . . .	4·02	4·04

#### CONVERSION OF BROMLAPACHONE INTO DIOXYHYDROLAPACHIC ACID.

As the result of several experiments 1·5 grams of finely-powdered bromlapachone and 75 cc. of a one per cent. solution of caustic potash were boiled together with an inverted condenser. The bromlapachone dissolved slowly to an intense carmine-red solution. At the end of fifteen minutes the boiling was discontinued and the solution filtered. Stray crystals of bromlapachone, which had escaped grinding and were consequently able to resist the action of the potash for a long time, were thus removed. It was observed that a small quantity of a green substance, formed by the action of the potash, was also retained on the filter paper. A slight excess of acetic acid was added to the filtered solution when cold, resulting in the immediate formation of a precipitate in small quantity, consisting of a somewhat resinous brown substance. This was removed as quickly as possible by filtration through a folded paper of comparatively large size. The filtration must be promptly accomplished, otherwise the new acid will commence to crystallize out before

the separation of the brown impurity has been effected. In a very minutes after acidifying the acid commences to be deposited from the now orange-red solution in compact crystalline grains. The separation occurs slowly, the solution becoming simultaneously lighter in color. In the course of a few hours the crystallization is complete and the compound can be filtered off, washed well with water and dried. The acid, as thus obtained, is almost pure: 1.5 grams bromlapachone gave rather more than one gram of the new acid. It was twice recrystallized from ninety-five per cent. alcohol, in which it is not very soluble, and analyzed. A qualitative test showed that in spite of the diluteness of the potash solution used the bromine had been removed.

1.867 gram gave .4442  $\text{CO}_2$  + .0989  $\text{H}_2\text{O}$ .

	Found.	Calculated for $\text{C}_{15}\text{H}_{16}\text{O}_5$ .
C, . . . . .	64.88	65.21
H, . . . . .	5.88	5.79

Dioxyhydrolapachic acid melts at  $181^\circ$ – $182^\circ$ . Like bromlapachone, it crystallizes from ninety-five per cent. alcohol in two apparently different forms. No special experiments were made to determine the conditions governing the formation of each variety of crystal and sometimes the one form, sometimes the other, would be obtained under apparently similar conditions. One modification (that analyzed) consists of small prismatic crystals, often grouped together in the form of compact stars; the other, of very fine long needles, crystallizing in tufts.

This acid, like oxyhydrolapachic and lapachic acids, forms stable salts which are not decomposed by carbon dioxide. The color of their solutions is similar to that of those of lapachic acid. The small amount of available material did not permit a more complete study of these salts. Dioxyhydrolapachic acid dissolves somewhat in water and is soluble in all ordinary solvents.

#### CONVERSION OF DIOXYHYDROLAPACHIC ACID INTO OXYLAPACHONE.

While dioxyhydrolapachic acid is perfectly stable under ordinary conditions and shows no inclination whatever to



pass into its anhydride, it forms oxylapachone with great readiness in contact with mineral acids.

To a solution of 1.36 grams of the acid \* in the smallest possible quantity of boiling 95 per cent. alcohol, 5 cc. of concentrated hydrochloric acid were added: the color of the solution changed to an intense orange-red. After boiling for one or two minutes the solution was diluted with water as long as a precipitate was produced. A considerable portion of the oxylapachone was thus precipitated in small orange-red needles. The filtrate, still orange in color, was evaporated until a crust of crystals had formed on its surface. They were separated from the solution when cool. Total yield, 1.15 grams. The substance was purified for analysis by recrystallization from dilute alcohol; it separated in red needles, fusing at  $201^{\circ}5$ , and closely resembling lapachone in appearance.

0.2507 gram gave 0.6401  $\text{CO}_2$  + 0.1228  $\text{H}_2\text{O}$ .

	Found.	Calculated for $\text{C}_{13}\text{H}_{14}\text{O}_4$ .
C, . . . . .	69.63	69.76
H, . . . . .	5.44	5.42

#### CONVERSION OF OXYLAPACHONE INTO DIOXYHYDROLAPACHIC ACID.

Oxylapachone, like lapachone and bromlapachone, is almost insoluble in alkalis in the cold; on boiling, however, it readily passes into solution. Thirty-five cc. of a one per cent. solution of caustic potash, and 1.35 grams oxylapachone, were heated for several minutes at the boiling point. The solution was then filtered, cooled and acidified with acetic acid. Crystals commenced to form almost immediately, but the separation was not complete for some hours. After crystallization from alcohol the compound fused at  $181^{\circ}$ – $182^{\circ}$ , and in other respects was found to be identical with dioxyhydrolapachic acid, obtained as above-described from bromlapachone. The yield of the crude substance was

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\* The whole of the substance available after a few preliminary experiments had been made on a still smaller scale.



almost theoretical; 1.35 grams gave 1.36 grams of the acid, 1.45 being required by theory.

1.834 gram gave .4390 CO<sub>2</sub> + .0958 H<sub>2</sub>O.

	Found	Calculated for C <sub>15</sub> H <sub>16</sub> O <sub>5</sub> .
C, . . . . .	65.28	65.21
H, . . . . .	5.80	5.79

The following is a list of the acids and their anhydrides discussed in this and the preceding paper:

Melting Point.	Acid.	Anhydride.	Melting Point.
139°	C <sub>10</sub> H <sub>4</sub> { $\begin{matrix} O_2 \\ CH = CH - C_3H_7 \\ OH \end{matrix}$ Lapachic Acid.		
125°	C <sub>10</sub> H <sub>4</sub> { $\begin{matrix} O_2 \\ CH_2 - CH(OH) - C_3H_7 \\ OH \end{matrix}$ Oxyhydrolapachic Acid.	C <sub>10</sub> H <sub>4</sub> { $\begin{matrix} O_2 \\ CH_2 - CH.C_3H_7 \\ O \end{matrix}$ Lapachone.	155-6°
181-2°	C <sub>10</sub> H <sub>4</sub> { $\begin{matrix} O_2 \\ CH(OH).CH(OH).C_3H_7 \\ OH \end{matrix}$ Dioxyhydrolapachic Acid.	C <sub>10</sub> H <sub>4</sub> { $\begin{matrix} O_2 \\ H(OH).CH.C_3H_7 \\ O \end{matrix}$ Oxylapachone.	201°
	Unknown.	C <sub>10</sub> H <sub>4</sub> { $\begin{matrix} O_2 \\ CHBr - CH.C_3H_7 \\ O \end{matrix}$ Bromlapachone.	138°

The acids are bright yellow in color, the anhydrides are orange or red. This distinction in color extends to other lapachic acid and lapachone derivatives which the author has obtained, but which have not yet been sufficiently studied to justify their introduction here.

## NOTE ON THE COMPOUND OF PYRROL WITH PICRIC ACID.

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By SAMUEL C. HOOKER.

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*[Read before the Chemical Section, February 18, 1890].*

In a short paper, devoted to some similar reactions of carbazol and pyrrol,\* the author described, nearly two years ago, a compound which he had obtained from pyrrol and picric acid, fusing at about  $71^{\circ}$  and crystallizing in long red needles. The method of formation, as well as the properties of this compound, justified the assumption that it was an addition-product similar to the picric acid compounds of indol and carbazol.

Owing to the great instability of the compound, its analysis promised considerable difficulty, and was consequently deferred until the required leisure would make it possible to devote to it the necessary time and care.

By following the directions here given, no difficulty will be found in preparing the compound in a pure form: Freshly-distilled pyrrol is added to a small quantity of picric acid, in quantity a little more than may be necessary to cover it. As soon as the pyrrol comes in contact with the acid, the latter becomes intensely red. The temperature, which must at no time exceed  $58^{\circ}$ , is then gradually raised until the picric acid is completely dissolved. The heating must not be continued longer than is actually necessary for the picric acid to pass into solution. A short time after the temperature has fallen the compound separates in red needles, some long, others short and broad.

If a sufficient quantity of pyrrol be used in proportion to the picric acid and care be taken to heat as short a time as possible, the crystals are very easily freed from the mother liquor by pressure between filter paper. If, on the other hand, these points are not attended to, the mother liquor is

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\* This *Journal*, 127, 63.

so thick and sticky that simple pressure does not suffice to leave the crystals in a pure condition.

To prepare the substance for analysis the mother liquor was drained off and the crystals were then rapidly and repeatedly pressed between porous paper until the paper was scarcely, or not at all, stained. The substance was then placed between well-fitting watch-glasses and immediately weighed. As soon as the crystals are dry, they commence to decompose, giving off pyrrol; they were, therefore, simply exposed to the air after the first weighing and then weighed again until the weight remained constant. The loss in weight represented the pyrrol given off, the residue consisted of picric acid.

(I)	·3230	gram	lost	·0740	gram.
(II)	·5350	"	"	·1210	"
(III)	·4460	"	"	·1015	"

	Found.			Calculated for
	I.	II.	III.	$C_4H_5N, C_6H_3N_3O_7$
Pyrrol, . . . . .	22·91	22·61	22·75	22·63
Picric acid, . . . . .	77·09	77·39	77·25	77·37

That pyrrol was really given off from the crystal was ascertained by holding a pine splinter, moistened with hydrochloric acid, in the neighborhood of crystals which had been exposed for two or three hours (presumed to have been sufficiently long to have allowed all possible traces of pyrrol simply adhering to the crystals to have escaped); the characteristic reaction was obtained.

The residue was of a very pale yellow color, and fused, at the same time darkening, at about  $120^\circ$ . It dissolved almost entirely in water, leaving a minute quantity of a dark resin. Crystallized once from water it fused\* at  $120^\circ$ – $121^\circ$ . The properties of the residue—its fusing point, the intense color and intensely bitter taste of its solution, its crystalline habits, etc.—corresponded exactly with those of picric acid.

Alcoholic solutions of pyrrol and picric acid deposit red crystals of the addition compound on spontaneous evaporation. The same is true of a chloroform solution. The combination does not appear to take place when benzol

\* The fusing point of picric acid is  $122^\circ\cdot5$ .

is the solvent employed.\* Considering the very unstable nature of the pyrrol compound, it is not difficult to understand how the slight affinity, which exists between pyrrol and picric acid, may be overcome in the presence of relatively large quantities of benzol which itself forms an unstable compound with picric acid.

### ON SOME CONSTANTS OF AMMONIA.†

BY DR. HANS VON STROMBECK.

[Read at the stated meeting of the Chemical Section, October 21, 1890.]

[Concluded from vol. c, p. 481.]

#### III.—DETERMINATION OF THE HEAT OF ABSORPTION OF AMMONIA.

The tests were made in the following way: The gas is developed by liquid ammonia contained in tank *AB* (Fig. 3). In order to have an as constant development of gas as possible during the test, tank *AB* is put in tank *CD* filled with water. Cock *Z* is of the same construction as cock *Z* in the preceding tests. The gas has to pass through the low stratum of mercury in the glass tube *EF*, and so its velocity can be controlled exactly. From hence the ammonia gas has to pass through the glass-coils *GH*. By passing through these coils lying in tank *IK*, the water of which is always renewed, the gas enters the absorption tank *NOPQ* at an almost constant temperature. The pipe *RS* reaches almost to the bottom of the tank and dips into a small glass tube secured in a piece of cork and filled with mercury, in order to prevent the water from rushing up in this pipe after it came in contact with the ammonia.

In each test 11,000 grammes of water are put in the absorption tank; its value in water being 245 grs., the weight of water (*M*) which is to be heated up is 11,245 grs. The quantity of ammonia gas (*P*) absorbed by the water is obtained by taking at the end of the test 100<sup>ccm</sup> from the

\* M. Dennstedt, *Ber. d. chem. Ges.*, **21**, 34, 31.

† From the laboratory of the De La Vergne Refrigerating Machine Co.

absorption tank, determining its contents of ammonia and multiplying the figure by 110.

I shall add to each item the figures obtained in the second test.

If in the following:

$P$  is the weight of ammonia which was absorbed (108.67 grs.);

$\theta$  is the temperature of the water in the absorption tank at the commencement of the test (15° 88);

$\theta_1 + \delta$  is the corrected temperature of the water in the absorption tank at the end of the test (20° 79);

$\frac{\theta + \theta_1}{2}$  is the average temperature prevailing in the absorption tank during the test (17° 84);

$\frac{\tau + \tau_1}{2}$  is the average temperature of the ammonia on entering the absorption tank (17° 31);

$\frac{\theta + \theta_1}{2} - \frac{\tau + \tau_1}{2}$  is the difference of both temperatures (0° 53);

$\theta - (\theta_1 + \delta)$  is the number of degrees by which  $M$  was heated up (4° 88);

$c$  is the specific heat of gaseous ammonia (0.5084).

The heat ( $Q$ ) developed by  $P$  is

$$M[\theta - (\theta_1 + \delta)] = 54875.6 \text{ thermal units.}$$

But we have to make one correction, it is to be added:

The quantity of heat ( $s$ ) required by  $P$  for heating itself up from

$$\frac{\tau + \tau_1}{2} \text{ to } \frac{\theta + \theta_1}{2}; \quad s = Pc \left( \frac{\theta + \theta_1}{2} - \frac{\tau + \tau_1}{2} \right) = 29.3$$

Consequently we have

$$Q = 54875.6 + 29.3 = 54904.9$$

or the heat developed by the unit ( $\mu$ ) is equal to

$$\frac{Q}{P} = 505.3 \text{ thermal units.}$$



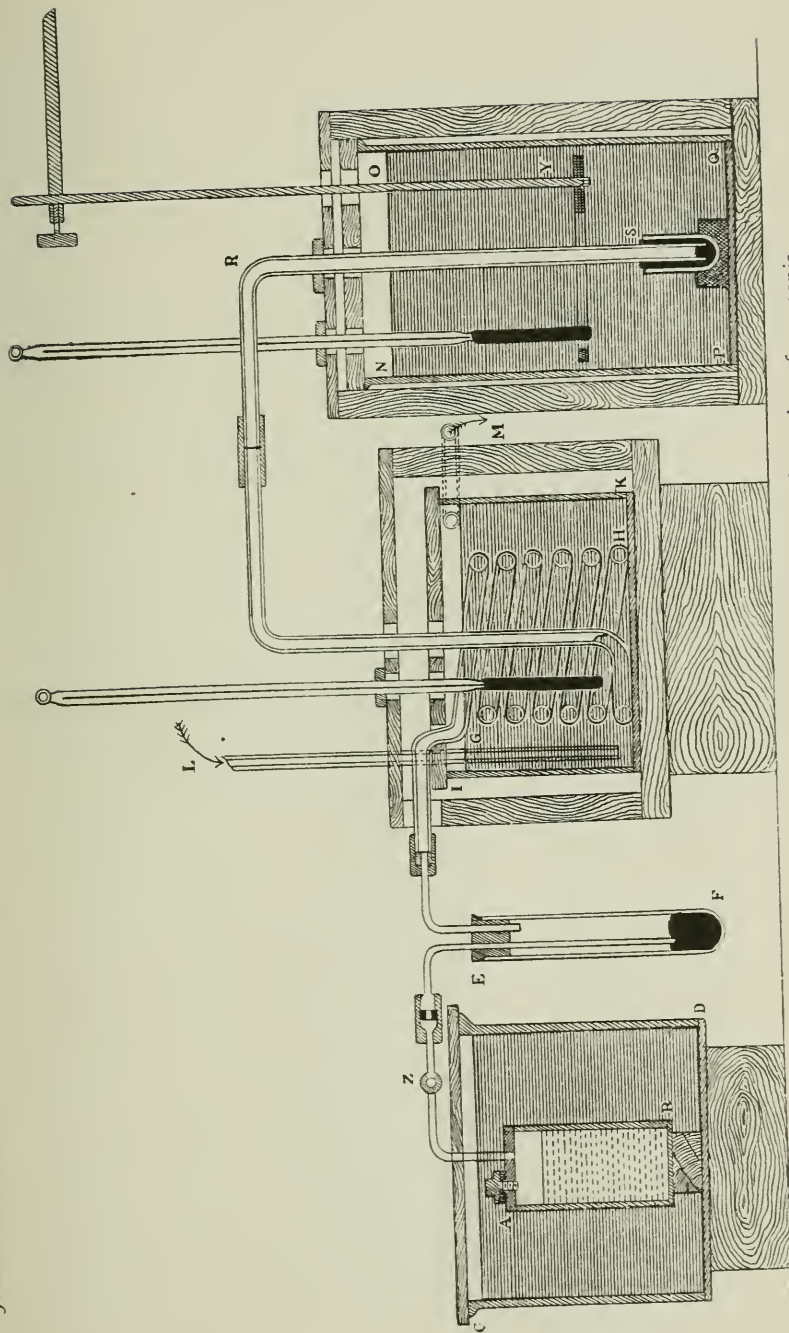


Fig. 3.—Apparatus for the determination of the heat of absorption of ammonia.





## AUTHENTICAL PROOFS OF THE DETERMINATION OF THE HEAT OF ABSORPTION OF AMMONIA.

	1.	2.	3.	4.	5.	6.	7.	8.	AVERAGE.
$P$ , . . . . .	112'25	108'67	110'1	148'5	134'0	100'54	152'2	170'5	
$M$ , . . . . .	11245'0	11245'0	11245'0	11245'0	11245'0	11245'0	11245'0	11245'0	
$\theta$ , . . . . .	13'89	15'88	15'33	12'88	13'83	15'91	12'66	12'58	
$\theta_1 + \hat{\theta}$ , . . . . .	19'20	20'79	20'31	19'68	20'06	20'61	19'80	20'65	
$\frac{\theta + \theta_1}{2}$ , . . . . .	15'52	17'84	16'75	15'55	17'01	18'09	15'91	16'23	16'061
$\frac{\tau + \tau_1}{2}$ , . . . . .	14'693	17'31	15'59	14'96	15'85	16'06	15'27	16'00	
$\frac{\theta - \theta_1 - \tau + \tau_1}{2}$ , . . . . .	0'827	0'53	1'16	0'59	1'26	2'03	0'64	0'23	
$\frac{\theta - (\theta_1 + \hat{\theta})}{2}$ , . . . . .	5'175	4'88	4'7009	6'6315	5'944	4'5281	6'4704	7'4446	
$M(\theta - (\theta_1 + \hat{\theta}))$ , . . . . .	58192'9	54875'6	55110'6	74571'2	66840'3	50918'5	75008'6	83714'5	
$S$ , . . . . .	47'3	29'3	66'3	46'1	87'7	104'1	47'7	18'6	
$Q$ , . . . . .	58241'4	54904'9	55177'9	74618'7	66928'6	51023'6	75057'7	83734'4	
$\mu = \frac{Q}{T}$ , . . . . .	518'8	505'3	501'1	502'3	499'1	507'7	493'1	491'1	{ 502'3 French or 904'1 Engl. thermal units.

## IV. DETERMINATION OF THE HEAT OF COMBINATION OF LIQUID AMMONIA AND WATER.

The tests were made in the following way: To tank *A* (*Fig. 4*) which through the hole *c* is to be filled with ammonia *P* is secured the pipe *R*, which has at its end the cock *E* and the union *O*; cock *E* can be handled by *G*. Union *O* can be connected with union *N*, terminating into the bottom of tank *B*, which is to be filled with water. Support *F* serves the purpose of making the whole apparatus steadier. I first made the connection between the upper ammonia- and the lower water-tank by a straight pipe going from the bottom of the upper to the top of the lower tank. In its middle a cock was placed which could be handled in the same way as the now present cock. But in this case at the place of contact between both fluids, a saturated stratum of aqueous ammonia is formed which, being heavier than liquid ammonia and lighter than water, does not move at all, but stays where it is, thus preventing both fluids from combining with one another. On the other hand, it will not do to put the liquid ammonia in the lower tank and the water in the upper one, for, it being next to impossible to fill a tank entirely with a fluid of such a low boiling point as liquid ammonia, there would always be left a small space filled with gaseous ammonia which, of course, would be at the top of the lower tank. Consequently, we would not determine the heat of combination but would again determine the heat of absorption.

In the following I shall add to each item the figures obtained in the first of the five tests I made.

Tank *A* is weighed empty, cocks *E* and *D* being closed, and *L* shut by a plug. Then it is filled with as much liquid ammonia as possible and weighed again. The difference between both weights is the one of ammonia *P* (53.7 grs.). At the end of each test it remains filled with ammonia gas ( $P - P_1$ ) at atmospheric pressure. The contents of *A* being 160<sup>ccm</sup>,  $P - P_1$  weighs  $160 \times 0.0007614 = 0.1218$  grs., which are to be subtracted from *P*, so as to obtain  $P_1$  (53.58 grs.). After *A* is filled it and tank *B* are put in a large vessel with water, and after, by proper manipulations, *B* as well as the

connecting pipe between *A* and *B* are entirely filled with water, both are connected. The whole apparatus being put in tank *K* filled with a certain quantity of distilled water, *L* is connected with a gauge after the plug is removed. The agitator *I* is put in motion, and the thermometer and the gauge are observed. As soon as for about five minutes the temperature and the pressure have become almost constant, cock *E* is opened. The pressure goes rapidly down to the atmosphere, and the thermometer ascends quickly, the temperature indicated by it soon becoming almost constant. From the figures thus obtained the heat of combining ( $\phi$ ) can be calculated.

The quantity of water (*M*) which is to be heated up in each test, consists of:

	<i>grs.</i>
The value in water of the apparatus, . . .	567.4
The weight of the water in tank <i>B</i> , . . .	237.0
The weight of the water in tank <i>K</i> , . . .	8200.0
The weight of the liquid ammonium in tank	
<i>A</i> , multiplied by its specific heat, . . .	$53.58 \times 1.22876 = 65.8$
	<hr/> <i>M</i> = 9070.2

If

$\theta$  = the temperature of the water in tank *K* at the commencement of the test ( $20^{\circ}.60$ );

$\theta_1$  = the temperature of the water in tank *K* at the end of the test ( $21^{\circ}.77$ );

$\theta - \theta_1$  = the difference between both temperatures ( $1^{\circ}.170$ );

$p^{\theta}$  = the absolute pressure in atmospheres prevailing at the commencement of the test ( $8.69$ );

$p^{\theta_1}$  = the absolute pressure in atmospheres prevailing at the end of the test ( $1.0$ );

the heat developed by the combining of  $P_1$  (53.58 grs.) with water is equal to  $M(\theta - \theta_1) = 10612.1$  or the heat ( $\phi$ ) developed by the unit is

$$\phi = \frac{M(\theta - \theta_1)}{P_1} = 198.1 \text{ thermal units.}$$

I did not take into consideration the difference of weight

between the few cubic centimeters of gaseous ammonia being over the liquid at the commencement of the test at  $p^\theta$  and the 160<sup>ccm</sup> of gaseous ammonia filling tank *A* at the end of the test at  $p^{\theta_1}$ , because its influence on the result, if any, can only be minimal.

According to the equivalents of  $NH_3$ ,  $H_2O$ , 17 grs. of the former combine with 18 grs. of the latter to 35 grs. of  $NH_4(OH)$ . Therefore, from a theoretical point of view, it would be best to use ammonia and water in these proportions in a test. But, because of technical reasons, the surface of contact between ammonia and water in an apparatus can be only small, so that the diffusion between both will take place rather quickly only then if the specific gravity of the resulting aqueous ammonia is a great deal greater than the one of liquid ammonia; for in this case there is always in *B* a descending current of aqueous ammonia and an ascending current of liquid ammonia, and fresh water comes always into contact with fresh ammonia. I, therefore, took the dimensions of tanks *A* and *B* in such a way that the ratio between the combining ammonia and water was, instead of 17 : 18, only about 8 : 18.

The results obtained in the other four tests were :

	2.	3.	4.	5.	Average of all 5 tests.
$P$ , . . . . .	59'2 grs.	60'22	55'12	60'20	
$P_1$ , . . . . .	59'08 grs.	60'10	55'00	60'08	
$P - P_1$ , . . . . .	0'12 grs.	0'12	0'12	0'12	
$M$ , . . . . .	9276'5 grs.	9277'7	9271'1	9277'7	
$\theta$ , . . . . .	21'0'29	23'0'94	19'0'62	21'0'16	
$\theta_1$ , . . . . .	22'0'48	25'0'29	20'0'84	22'0'47	
$\theta - \theta_1$ , . . . . .	1'0'19	1'0'35	1'0'22	1'0'31	
$M(\theta - \theta_1)$ , . . . .	11039'0	12524'9	11310'7	12153'9	
$p^\theta$ , . . . . .	8'75 atm.	9'68	8'39	8'52	
$p^{\theta_1}$ , . . . . .	I	I	I	I	
$\phi$ , . . . . .	186'9	208'4	205'6	202'3	{ 200'3 French or 360'5 Eng. thermal units.



V. TABLE, *Showing the amount in French and English thermal units required or developed by liquid or gaseous ammonia when changing their respective states of aggregate and for what these thermal units are used.*

Total heat required by liquid ammonia to transform itself at 17°0 (62°60 F.) into saturated vapor, its boiling point being in reality at — 0°86 (30°45 F.), . . . . . — 318·8 (573·8)	{	— 296·8 (534·2) heat of evaporation.
		— 22·0 (39·6) heat of liquid.
Heat developed by the absorption of gaseous ammonia by water at atmospheric pressure, the temperature of the resulting aqueous ammonia being 16°6 (61°9 F.), . . + 502·3 (904·1)	{	+ 318·8 (573·8) to become a liquid at — 0°86.
		+ 46·2 (83·2) to cool itself down from — 0°86° to its boiling point at atmospheric pressure to — 38°5 = 37·64 × 1·22876.
		— 67·6 (121·7) to heat itself up from — 38°5 to 16°6 = 55·1 × 1·22876.
		+ 200·3 (360·5) for chemical work done by the combination of liquid ammonia and water.
		_____
		+ 497·7 (895·8)

## BOOK NOTICES.

*Handy lists of technical literature.*—Reference catalogue of books printed in English from 1880 to 1888, inclusive, to which is added a select list of books printed before 1880 and still kept on publishers' and jobbers' lists. Part II<sup>a</sup>. Electricity and magnetism, telegraph, gas, etc. Compiled by H. E. Haferkorn, Milwaukee. 1890.

This, the fourth part of the valuable set of handy lists, is arranged on the same basis as former ones, including a large number of cross-references, which aid greatly in finding any particular work; there are also notes and remarks, giving such information as is necessary to complete each entry. The entire catalogue is supplemented by a list of newspapers, periodicals, almanacs and annuals.

Being the first attempt made at cataloguing the later literature of this extensive subject, we are surprised to find it so complete.

It will recommend itself to all who are interested in electricity, magnetism or gas. R.

*Sugar analysis.* For refineries, sugar houses, experimental stations, etc., and as a hand-book of instruction in schools of chemical technology. By Ferdinand G. Wiechmann, Ph.D. New York: John Wiley & Sons. 1890. Price, \$2.50.

This book forms a valuable supplement to the works on sugar analysis already in use. Some important matters, however, have been insufficiently



discussed, while others have been entirely overlooked. One brief page is devoted to the methods of examining how effectively sugar has been decolorized in passing through bone-black ; but a description of the process of decolorizing sugar for comparative tests, which should precede this, has not been given.

Bone-black itself has, it is much to be regretted, been almost entirely passed over. On p. 107, we are shown how the weight of a cubic foot of this substance may be ascertained ; but with this exception there is little or nothing said about it. It is true that the analysis of bone-black is foreign to the subject-title, but in view of the almost inseparable connection of sugar and bone-black, the value of the work would have been much increased, both to the practising chemist and also as a "hand-book of instruction in schools of chemical technology," if some space had been devoted to it. We find no mention of either the sugar-cane or the sugar-beet ; but this we regard as a very important omission. A chapter might advantageously have been devoted to the detection of foreign substances in sugar. Thus, a description of the best methods of finding tin, ultramarine, the various artificial coloring matters, etc., would have added to the value of the book.

After recording these and passing over similar omissions, we can highly commend the book to all interested in sugar analysis, as bringing down the subject to the present date in a concise and practical form. The book contains a valuable and very complete set of tables. S. C. H.

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## Franklin Institute

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[*Proceedings of the stated meeting, held Wednesday, December 17, 1890.*]

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, December 17, 1890.

JOS. M. WILSON, president, in the chair.

Present, 192 members and forty-three visitors.

Additions to membership since last report, forty.

The actuary, by direction of the board of managers, reported that the following persons, viz : C. O. C. Billberg, David Brooks, Henry Crew, Richard W. Davids, N. H. Edgerton, Richard W. Gilpin, C. Hanford Henderson, Carl Hering, Herman S. Hering, Edwin J. Houston, L. F. Rondinella, Pedro G. Salom, E. Alexander Scott, William H. Wahl, S. Lloyd Wiegand and Paul A. M. Winand, having applied to the board of managers to be constituted the electrical section of the Franklin Institute, and the said application having been approved, they are, in accordance with section 3, of article XI of the by-laws, hereby reported to the institute as the founders of the section.

He reported, further, that the following resolution was adopted by the board at its stated meeting, held December 10, 1890, viz :

"Resolved, that the board of managers hereby nominates to the institute for election as an honorary member, Dr. Francis Fowler, of Washington, D. C."

Dr. Fowler, thereupon, was elected as an honorary member.

The following nominations were then made for officers, managers, and members of the committee on science and the arts, viz :

For <i>president</i>	(to serve one year), . .	JOSEPH M. WILSON.
" <i>vice-president</i>	( " three " ), . .	WM. P. TATHAM.
" <i>secretary</i>	( " one " ), . .	WM. H. WAHL.
" <i>treasurer</i> ,	( " " " ), . .	SAMUEL SARTAIN.
" <i>auditor</i> ,	( " three " ), . .	WM. O. GRIGGS.

For *managers* (to serve three years) :

HUGO BILGRAM,	HENRY BOWER,
CYRUS CHAMBERS, JR.,	CHAS. H. CRAMP,
G. M. ELDRIDGE,	H. W. JAYNE,
H. R. HEYL,	WM. E. LOCKWOOD,
C. H. HUTCHINSON,	FRED. B. MILES,
S. R. MARSHALL,	HENRY PEMBERTON, JR.,
C. E. RONALDSON,	H. W. SPANGLER.
WM. SELLERS,	

For members of the *committee on science and the arts* (to serve three years):

JOHN E. CODMAN,	WM. D. MARKS,
THOS. P. CONARD,	PHILIP PISTOR,
C. B. DUDLEY,	HENRY PEMBERTON, JR.,
WM. C. HEAD,"	THOMAS SHAW,
C. J. HEXAMER,	L. H. SPELLIER,
H. R. HEYL,	S. P. SADTLER,
FRED. E. IVES,	T. C. SEARCH,
W. M. MCALLISTER,	WM. E. LOCKWOOD.

Mr. E. H. Johnson, of New York, read a paper describing the system of the Interior Conduit and Insulation Company, and showed the utility of the system by an experimental demonstration. (Referred for publication.)

Mr. John A. Wilson gave an account of the Philadelphia and Reading Railroad Company's terminal and elevated railway plans, describing, with the aid of colored sketches, lantern slides and detail drawings, the routes to be followed by the proposed line, and the character of the various portions of the elevated structure, buildings, crossings, etc. The subject evoked a lengthy discussion, which was participated in by Messrs. Le Van, Wiegand, Brown (Ed.), Watson, Garrison, Haupt and others. (The subject is referred for publication.)

Mr. W. N. Jennings exhibited the concluding portion of a series of photographic views, illustrating his trip to the Pacific Coast.

The secretary read the following communication, received from Mr. J. E. Watkins, of the U. S. National Museum, viz :

## CIRCULAR NO. I.

Central committee for the celebration of the beginning of the second century of the American patent system by inventors and manufacturers of patented inventions.

Executive Committee.—Hon. John Lynch, Chairman ; J. Elfreth Watkins, Secretary ; J. W. Babson, M. C. Stone, G. C. Maynard.

—  
OFFICE, BOARD OF TRADE, 1419 F STREET, N. W.  
WASHINGTON, D. C., December 12, 1890.

*To the inventors of America and the manufacturers of inventions :*

The completion of the first century of the American patent system marks so important an epoch in the history of the nation that it is eminently proper that the beginning of the second shall not pass unnoticed.

The centennial anniversaries of other important national events have been celebrated in a manner worthy of a people proud of their country and its growth. Surely, the system that has aided the agriculturalist in the field, the mechanic in the shop, and the toiler in the mine ; that has stimulated invention, and protected the rights of the inventor and his most important ally, the manufacturer, has played no small part in a history so full of the triumphs of human achievements.

Believing that the American inventor and manufacturer of inventions will regard it a privilege, as well as a duty, to co-operate in making due recognition of these facts, it is proposed to hold a celebration at the national capital, in April next, which shall in a fitting manner commemorate the important event and place on record that nation's appreciation of the labors of those whose ingenuity, patience and tireless effort have exercised such a potent influence in accelerating the prosperous growth of the nation, and in aiding the progress of our civilization.

The necessity for a national association of inventors and manufacturers of inventions organized for mutual benefit has frequently been discussed in the technical and other journals. No time could be more opportune for the formation of such an association than when men from every part of the country meet to celebrate so important an anniversary.

Surely, the occasion is most inspiring.

All inventors and manufacturers and others interested are requested to co-operate with this committee in the purpose above set forth. Correspondence appertaining thereto should be addressed to

J. ELFRETH WATKINS,  
*Secretary U. S. National Museum.*

Mr. S. L. Wiegand spoke in favorable terms of the system of rolling steel tubes from the solid, devised by the Mannesmann brothers, and described at the previous meeting, and moved that the invention be referred to the committee on science and the arts, with the view of recognizing the merits of the same by some suitable award. Carried.

Adjourned.

WM. H. WAHL, *Secretary.*

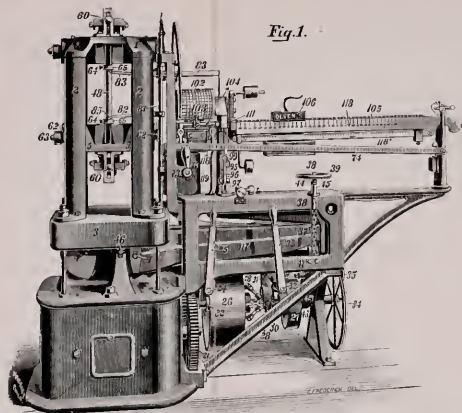


Fig. 1.

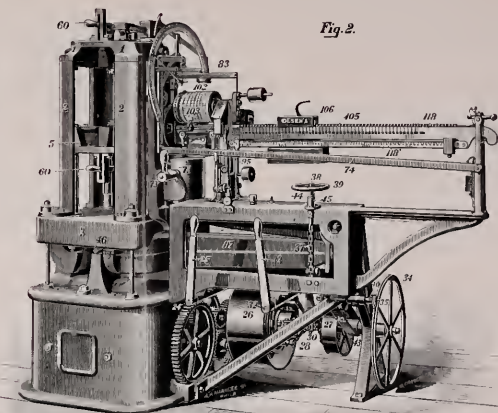


Fig. 2.

- 1, Entablature.
- 2, Columns.
- 3, Platform supporting columns.
- 4, Pivots.
- 5, Lower moving head.
- 14, Spur wheel.
- 16, Idle wheel.
- 19, Pinion.
- 21, Spur wheel on sleeve 22.
- 22, Sleeve on driving shaft.
- 24, Rock shaft operating lever shifting 22.
- 25, Hand lever operating 24.
- 26, 27, Pulleys rotating driving shaft.
- 28, 29, Friction clutches engaging 26 with driving shaft.
- 30, Sleeve operating clutches.

- 31, Forked lever controlling sleeve 30.
- 32, Shaft.
- 33, Hand lever operating 30.
- 34, Grooved wheel on driving shaft.
- 35, Small wheel turning 34.
- 36, Arbor to 35.
- 37, Chain.
- 38, Adjusting screw.
- 39, Nut.
- 40, Tilting bearing.
- 41, Band wheel.
- 42, Endless band.
- 43, Pulley turning with pulley 27.
- 44, Helical spring.
- 45, Support for 44.

- 46, Fulcrum of lever 117.
- 48, Specimen under test.
- 49, Gripping jaws.
- 50, Projecting flanges on jaws 49.
- 51, Block slide.
- 52, Grooves in 51.
- 53, Slotted slide supporting 49.
- 54, Opening in 53.
- 55, Eye in 53.
- 56, Bolt connecting 53 and 57.
- 57, Lever to open and shut jaws.
- 58, Fulcrum of 57.
- 59, Counterweight.
- 60, Handle of lever 57.
- 61, Plungers for slides 51.

EXPLANATION OF REFERENCE NUMBERS.

- 62, Screws for
- 63, Screw bolt.
- 64, Collars or c
- 65, Set-screws a
- 66, Detachable
- 67, Tenons fitti
- 68, Guiding pla
- 69, Screws to 6
- 70, Nuts to scre
- 71, Washers to
- 72, Guiding bla
- 73, Cant.
- 73', Handle of
- 73'', Grooves i
- 74, Lever movi

# THE OLSEN TESTING MACHINE.

(Com. Sci. and the Arts.)

Fig. 2.

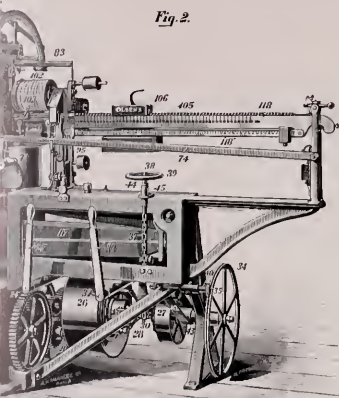
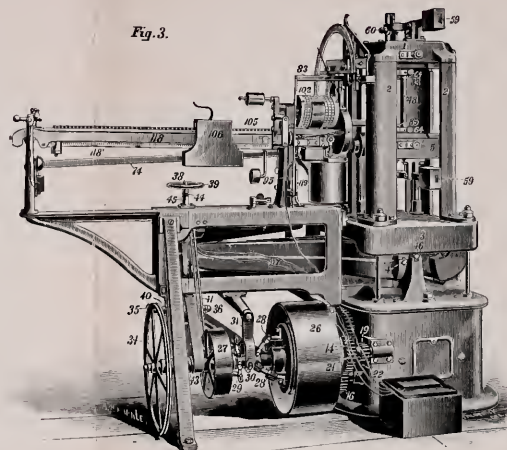


Fig. 3.



## EXPLANATION OF REFERENCE NUMBERS.

- 46, Fulcrum of lever 117.
- 48, Specimen under test.
- 49, Gripping jaws.
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- 53, Slotted slide supporting 49.
- 54, Opening in 53.
- 55, Eye in 53.
- 56, Bolt connecting 53 and 57.
- 57, Lever to open and shut jaws.
- 58, Counterweight.
- 59, Handle of lever 57.
- 60, Plungers for slides 51.

- 62, Screws for 61.
- 63, Screw bolt.
- 64, Collars or clamps for caliper bearing.
- 65, Set-screws in 64.
- 66, Detachable cap on clamps 64.
- 67, Tenons fitting 66.
- 68, Guiding plates.
- 69, Screws to 63.
- 70, Nuts to screws 69.
- 71, Washers to 69.
- 72, Guiding-block.
- 73, Cam.
- 73', Handle of cam.
- 73'', Grooves in clamps 64.
- 74, Lever moving 87.

- 75, Sliding-blocks.
- 76, Supporting-guide to 75.
- 77, Set-screws for 75.
- 78, Polygonal prism in 75.
- 79, Shaft for 78.
- 81, Pins for holding 79
- 82, Calipers.
- 85, Arm of caliper.
- 86, Clamps.
- 87, 88, Upright bars or rods.
- 95, Cord operating recording cylinder.
- 96, Pulley.
- 97, Lever.
- 98, Fulcrum to 97.
- 99, Pulley or sheave.

Fig. 4.

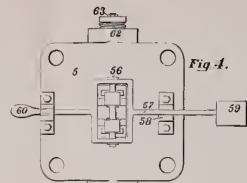


Fig. 5.

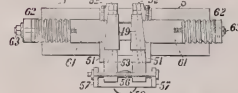


Fig. 6.



Fig. 7.



Fig. 8.



Fig. 9.

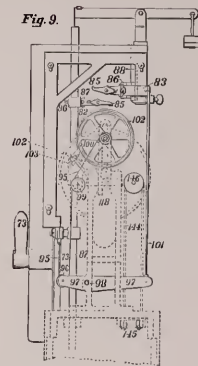


Fig. 11.

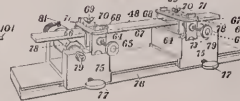
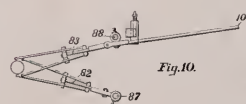


Fig. 10.



- 100, Drum or winding-barrel of 102.
- 101, Link.
- 102, Recording cylinder.
- 103, Pencil.
- 104, Screw.
- 105, Screws shifting 106.
- 106, Poise or weight.
- 111, Balancing pivot of beam.

- 117, Force multiplying lever.
- 118, Weighing beam.
- 118', Slide to small poise on 118.
- 119, Link.
- 144, Endless band for moving poise.
- 145, Guiding pulleys.
- 146, Grooved wheel.



# JOURNAL

OF THE

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OF THE STATE OF PENNSYLVANIA,

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VOL. CXXXI.      FEBRUARY, 1891.

No. 2

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## THE OLSEN TESTING MACHINE.

[*With abstract of the report of the Committee on Science and the Arts thereon.*]

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The Olsen testing machine is the result of continued efforts of the inventor to improve his original machine and to meet the additional requirements demanded by progress in the arts of metallurgy and constructions in metal. The earlier investigators in these arts were careful to ascertain the ultimate strength of materials; afterwards a knowledge of the properties of elasticity and ductility were deemed important and were crudely examined into by bending specimens under the hammer. As the art of making structural work progressed, these properties became of importance in estimating the strength and durability procurable by selections of material and questions of cost were affected by such selections. Exact ascertainment and expressions of all of the properties, ultimate strength, limits of elasticity, both in dimensions and in force, and



limits of ductility, expressed in force and dimension, were all demanded for intelligently applying materials to use, as well as for comparing the values of different materials.

The mode of testing these properties or functions of materials under strain, by weighing, and ocularly observing the weight and measuring from time to time by hand the variations in distance of marks previously made upon the specimen, as they changed under the stress applied, was found to be too slow and uncertain, and, therefore, very unsatisfactory, first, because there were so many tests required and the making of them was too long and tedious, and second, the changes which took place during tests occurred in such rapid succession as often to defy accurate observation.

Mr. Olsen's machine is designed to meet these requirements and furnish easy means of prompt application of the specimens to the machine, and of adjustment of the machine to the specimens, with the least requirements in preparation.

To give a clear conception of this machine, it is well to view the instrumentalities by which each function is performed, and afterwards state their combined operation.

First in order is the gripping mechanism for holding the specimen to try its properties under extension.

This consists of wedges, which, under the strain of pulling, are forced toward each other by passing into converging spaces in the draw-heads. To these wedges are attached links, levers and a counterbalance, which enable the operator easily to open and close them, to insert the specimens, before stress is applied, and plungers with adjusting screws for varying the width of the converging space in which the jaws are drawn, to close them so as to adapt them to specimens of different thicknesses.

Second, the means for adjusting the distance of the draw-heads and applying stress and of varying and of graduating the rate of applying the stress.

These consist of a series of four screws attached to the lower draw-head containing the lower set of gripping jaws, which screws and connected parts are drawn downward by nuts turned by a series of toothed wheels and pinions enclosed

in the base of the machine. This train of wheels and pinions is propelled by a shaft projecting from the side of the base and bearing a large spur wheel, which may be actuated rapidly by an idle wheel turned by a large pinion and intermediate upon the shaft bearing the driving pulleys, or, by sliding the large pinion out of gear from the driving shaft and engaging a small pinion in gear with a large spur wheel which turns with it upon the same axis and turns a small pinion engaging in the large spur wheel on the side of the base. This last adjustment effects a great multiplication of force with a corresponding decrease of motion of the screws and connected draw-head.

In addition to this provision for changes of speed, there are placed in the driving shaft a large and a small band-wheel, either of which may, by friction clutches, be alternatively engaged with the driving shaft. By means of these the shaft can be turned rapidly for the purpose of applying small force and quickly moving the draw-head into position to receive the specimens, or with less speed and with greater force, to apply stress to the specimens.

Thus far as described, there are four different adjustments of speed and force in applying motion to the draw-head, all of which are prompt and positive in their action. There is yet another, susceptible of fine graduation and instant control. This is effected by a large grooved friction wheel upon the end of the driving shaft, that may at will be turned by a small wheel upon a small shaft, in constant motion, and that can be pressed into constant engagement and released by the hand of the operator by means of a nut in a small hand wheel controlling a screw attached to the bearing of the small shaft, so that the operator can feel and observe the strain as he gradually applies it.

The third system of mechanism is that used for weighing the strains.

This consists of a frame or table supported on levers with knife-edge fulcrums, in principle substantially like those of a platform scale. Upon this table, the entablature containing the upper draw-head is supported by four columns. The levers for supporting the table and upper

draw-head are connected, by an intermediate lever and links having knife-edged bearings throughout, to a graduated beam, upon which a poise slides outwardly from the fulcrum as it is propelled by a screw supported in a parallel position on the beam, and turned by means of an endless cord that passes to the screw-propelling wheel in a vertical plane coincident with the balancing knife-edge. The engagement and disengagement of the screw-driving wheel with the screw are controlled by an electro-magnet having helices in circuit with electrodes, one of which is located on the outer end of the beam, and the other above it, so that whenever the beam rises, the circuit is closed and the poises on the beam moved further from the fulcrum. Then as further stress is applied, if the beam again rises, the poise is moved still further from the fulcrum, increasing the stress.

The rate of the progressive motion of the poise upon the beam is made variable and adjustable by means of a series of intermediate friction wheels through which the endless band receives motion. The first of this series of wheels receives a uniform motion from an endless band or cord passing around a grooved pulley turning with one of the driving pulleys in the driving shaft of the stress mechanism.

The motion of the pulley is transmitted by a slightly conical plate wheel embraced elastically between a pair of plate wheels having an adjustable arbor. The opposite side of the pair of plate wheels embraces another single, slightly conical, plate wheel turning the grooved wheel driving the endless cord leading to the screw-propelling pulley. The rate of motion of the cord and screw-driving pulley is less when the propelling plate wheel engages at its smaller diameter with the pair of plate wheels and when the opposite side of the pair of plate wheels engages the larger diameter of the driven plate wheel, and is greatest when the greater diameter of the driving plate wheel engages the pair of plate wheels, whilst the opposite edges of the pair of plate wheels engage the smaller diameter of the driven plate wheel. The fulcrum of the pair of plate wheels being adjustable as to position between the driving and driven

conical plate wheels, any desired intermediate rate of progression in weighing is readily procured.

The fourth system of mechanism embodied in this machine is that for measuring changes of form and recording the same, together with the stress upon the specimen at each and every instant of change.

This consists in a pair of clamps or divided collars which are secured by pointed set screws elastically held upon the specimen at definite initial distances, and calipers resting upon these collars, which, by means of an intervening mechanism, rotate a diagram-sheet holding cylinder, exactly in proportion to the differences in the motion of the opposite caliper jaws, and a pencil resting upon and marking the diagram sheet, travelling in the direction of the axis of the cylinder, in exact ratio with the motion of the poise upon the beam.

This portion of the mechanism is so novel and dissimilar from preceding inventions for the purpose as to require more exact and detailed description.

The clamps are two frames of metal having a pointed set screw and an opposite pointed plunger in line with and pressed toward the point of the set screw by a helical spring. A plate, fitting with mortises over tenons in the ends of the open sides of the clamp, serves to close it and support an adjustable block held by a thumb screw through a slot in the plate, which block, resting against the specimen, prevents any undue rocking of the clamp upon the points of the set screw and pointed plunger.

A pair of saddles mounted adjustably as to distance upon a parallel-sided bar and provided with seats to receive the lower sides of the clamps, serves to gauge them as to distance when placing the specimen in them; and in conjunction with each saddle, there is a set of rotatable gauges resembling eccentric polygonal prisms, which, by being turned to proper adjustment, support the specimen so that the plunger point and screw point lie in the same plane as the central axis of the specimen.

This apparatus for adjusting the clamps or collars upon the specimen is, of course, a distinct and separate implement



from the machine, but being an extremely useful, if not indispensable, adjunct, should be described with it.

The specimen with the clamps upon it is placed with its ends between the jaws in the draw-heads of the machine and the jaws tightened upon it by hand levers. Under the upper collar and above the lower collar, the fingers of a forked pair of calipers are placed. The fingers of the calipers are connected by pivots, and can be spread to adapt them to different widths of specimens and have an intermediate fulcrum upon which they can rock and adapt themselves to any inclination of the clamps.

One pair of caliper fingers is attached to an arm having a connected cord, which, after passing around a pulley in a lever, passes in a vertical plane coincident with the balancing axis of the beam, and is attached to a small winding barrel, secured to and turning the diagram-sheet bearing drum. The other pair of caliper fingers is connected by a link to the opposite end of the lever, and the arms of the lever are of such proportionate length to each other that the cord wound upon the barrel of the diagram-sheet holding drum is only moved to an extent proportioned to the difference between the motion of the caliper fingers. A slight spring in the winding barrel serves to wind the cord on the barrel and yields to the unwinding operation of the cord.

The pencil for marking the diagrams is moved by a connected nut engaged in the threads of a screw turned with the screw which operates the poise on the beam, so that the line marked on the paper indicates, by its ordinates, the stress at each moment noted by the line, and by the abscissæ, the extent of elongation between the points of attachment of the clamps.

In conjunction with the diagram-sheet holding cylinder there is an index, connected by a motion-multiplying device of sector and pinion, by which the elongation of the specimen is displayed upon a graduated sector or dial.

The fifth mechanism in this machine is an alarm or signal, by which, whenever the weighing beam oscillates beyond its normal extent of motion, a contact between an

electrode placed upon the beam and another placed above it, at greater distance than that controlling the poise motion, closes the circuit of an electric alarm bell and directs attention to the operation of the machine. This enables the operator when testing specimens cautiously and with slow applications of stress, to give attention to other work and yet witness every change of importance that might otherwise escape observation.

By removing the gripping jaws from the lower draw-head and placing supports in the table beneath it, and a block to transmit pressure from the draw-head to specimens laid upon the supports, the transverse strength or resistance to compression of specimens can be tested, and by introducing a mechanism between the draw-head for converting the right line of motion of the lever draw-head into curvilinear movement, the properties of materials under torsional strain can be tested, observed and recorded.

The combined effect of the several mechanisms is to enable the operator, with rapidity and certainty, to submit specimens to the action of the machine under conditions favorable for comparison of results, and to secure a graphic record of all the phenomena of change of form and variations in stress which occur at every stage of the test, to suit different dimensions of specimens and different rates of application of force to any required extent, and when desired, to closely feel the effect of the stress upon the specimen through the frictional gear with a delicacy of working, facility of observation and accuracy of record never before attempted.

This machine is the subject of United States letters-patent to Mr. Tinius Olsen, dated March 25, 1879 (No. 213,586); June 1, 1880 (No. 228,214); March 12, 1889 (No. 399,549) and January 27, 1891 (No. 445,476).

Following is an extract from the report of the Committee on Science and the Arts on the Olsen testing machine, to-wit:

"The committee recognizes that the increased complexity of this machine over others requires a more careful handling. They have not sufficient experience to say whether a greater number of tests can be made in a given



time than with other machines, but their opinion is that this testing method is a long step forward toward making such machines thorough instruments of precision, and it introduces instead of the numerical, the graphic record, the advantages of which are universally admitted.

"In view of the great ingenuity displayed by the inventor in arranging the several parts of the machine, notably in the mechanism which produces a graphic record of the test, similar to the indicator of a steam engine, and thus brings to perception at a single glance the variation in the strain of a number of specimens as well as the work required to break such specimens, the award of the Elliott Cresson medal is recommended."

GEORGE A. KOENIG,  
*Chairman.*

H. W. SPANGLER,

H. R. HEVL,

L. D'AURIA,

JOHN L. GILL, JR.

*Adopted December 3, 1890.*

H. W. SPANGLER,

*Chairman of the Committee on Science and the Arts.*

## ELECTRICITY: ITS PAST, PRESENT AND FUTURE.

BY RALPH W. POPE,  
Secretary American Institute of Electrical Engineers.

*[A lecture delivered before the Franklin Institute, December 16, 1889.]*

*[Concluded from p. 30.]*

It is not my purpose to trace the disputed ownership of the various inventions which serve to make up the modern systems of electric lighting. Probably every inventor who had given the subject any attention whatever, decided at once that the arc lamp was unsuitable for general interior lighting. The illuminating qualities of the arc lamp are due to the passage of the electric current from one carbon point to another. After the current begins to flow, these carbon points are separated and maintained at a uniform distance of about one-eighth of an inch by suitable

mechanism. The carbon pencils are slowly consumed by the action of the current, and it is necessary to renew them daily. The intense brilliancy of the arc lamp, its tendency to fluctuate, the hissing sound made by many of them, the high pressure of current generally used, are all objectionable features, when it is considered for domestic use. On the other hand, the incandescent lamp, as now made and used, is, without doubt, our most perfect artificial light. The heating property of electricity is utilized in this lamp, and the filament through which the current passes may be formed of different substances which, by patient investigation and experiment, have been found most suitable for the purpose. The metal platinum, which was first tried, has been replaced by carbon made from bamboo, silk fibre and other organic materials. In order to prevent the immediate combustion of the filament, it is fixed in a glass bulb, from which the air has been exhausted. Notwithstanding this precaution, however, the filament gradually deteriorates, and the average life of the lamp is limited to about 1,200 hours. With a perfect installation of incandescent lamps, there will be no fluctuation in the amount of light disseminated. There is a large and growing demand for lighting of this character, but unfortunately the necessity of power to drive a dynamo or the use of chemicals for generating electricity, prevents the extended use of the incandescent lamp among the masses. Its absolute safety, its healthfulness, its convenience, all point to its desirability for domestic use, but the high cost of its production restricts its enjoyment to a very small proportion of the people, while a large number of those who would gladly use it and willingly pay for it must wait patiently for the further development of the business. It is to be regretted that business competition, while stimulating the growth of the electric lighting business, has also brought it into disrepute by reason of the needless and entirely avoidable dangers which are supposed to be the necessary accompaniments of electrical distribution. The earlier electric lighting companies were apparently organized for speculative purposes. New York being the largest city in the country was the field naturally

selected for the exploitation of arc lighting on an extensive scale. The pioneer lines were constructed in the cheapest possible manner. It was soon ascertained by the insurance companies that the electric current used was liable to cause fires, and a system of inspection was organized by the Board of Underwriters, which had the authority to enforce it, as insurance of buildings was refused unless the installation conformed to the official standard of the board. Up to this time the wires had been covered with a cotton braid saturated with paraffine, under the supposition that they were thus insulated. A short exposure to weather soon removed this delusion, while it was found that the covering, upon being ignited, burned like a candle, the fire following the wire, thus increasing the risk of extensive conflagrations. This difficulty was obviated by the substitution of white lead paint for paraffine. This covering was fireproof and cheap. These were the only merits it possessed, but they were sufficient. It became known as "underwriters' wire," and following the lead of New York city, it has been extensively introduced in every important city and village in the country. It was soon discovered, however, that when water-soaked, the current would leak through this cotton covering wherever a contact was made that led to the ground, and at such points fires would be started, or that if a human being came in contact with the wire in such a manner as to become a path for the current, he was extremely liable to injury or death. The continual deterioration of these lines by exposure to weather, and the lack of attention given to their repair, led to the loss of so many lives that Superintendent I. N. Miller, of the Western Union Telegraph Company, suggested the name of "undertakers' wire," by which the description of wire approved by the Board of Underwriters is now generally known. It is unjust, however, to attribute all accidents to the use of this material. Had lines been properly constructed, and care taken to prevent accidental contacts, the record of deaths would not have been sufficiently large to have justified interference with the business on the part of the authorities. There is no such mystery about the action of electricity as is popularly supposed. We

provide a path over which it should travel, and if we make proper use of our scientific knowledge and engineering skill, the current may readily be confined to its prescribed limits. In the construction of lines, however, the electrical engineer has too frequently been ignored, and cheapness, rather than perfection, has been the end sought by those who are responsible for the management of most of these enterprises.

The use of electricity as a motive-power was investigated by some of our earlier inventors, as soon as the peculiar properties of the electro-magnet became known. It is true that the production of electricity by the chemical battery was a fatal obstacle to the commercial success of the electric motor in competition with the steam engine, but it was some time before the fallacy was entirely removed. Toy electric railways were exhibited throughout the country, and in 1837, Thomas Davenport, a village blacksmith of Brandon, Vt., operated a printing press by an electric motor, which like many of the electric inventions was really in advance of its time.

On April 29, 1851, Prof. Charles Grafton Page made a trial trip of his electro-magnetic locomotive, between Washington and Bladensburg, attaining a speed of nineteen miles per hour. An important improvement in electric motors was made by Pacinotti, in 1860, and although described in 1864, attracted but little attention at that time, as no sufficiently economical method of generating electricity had yet been discovered. This important feature existed, however, in the very machine which he had invented, and he subsequently pointed out that his principle was reversible, and that the motor might be used as a mechanical current generator. The advent of the dynamo machine in 1870, was followed by the practical demonstration in 1873, at the Vienna Exhibition, that a similar machine could be driven at a distance by simply connecting it into the circuit, and thus what is known as the reversibility of the dynamo, led up to the present activity in the field of electric power transmission and distribution. The most important branch of this field, and the most conspicuous, because it is brought directly to the



attention of the people, is the application of electricity to street railways. Those who are most interested in electrical progress are inclined to be impatient at what they believe is the tardy appreciation of the merits of electric traction. As a matter of fact electric railways have probably been introduced much more rapidly than was altogether prudent, when we consider the crude manner in which the work has been done in many cases, for it must be borne in mind that in the early stages of the development of a rival system, any failure is immediately utilized as an argument against the claims of the newcomer. We are all familiar with the manner in which accidents by electricity are reported throughout the land, and even cabled to Europe. Every effort will be used to remove the possibility of danger, and within ten years the electric motor will be as firmly established in our midst as the steam engine is to-day. Had it not been for the apparent danger from the boiler, the steam engine would probably have made its advent fifty years earlier, and civilization would have been correspondingly advanced. In 1769, as we learn from a German work, a locomotive for use on common roads was run in the streets of Paris by its inventor, but although it proved successful as a motor, it was unfortunate in its experience, for upon its first trial it punched a hole through a wall, and on its second it was upset and burst, and we can well imagine the alarm of the public at the idea of such a monster running amuck through the streets. Every day in the week, New York city is continually traversed by 300 high-pressure locomotives carrying steam at (say) 140 pounds to the square inch. The streets of our large cities are undermined with boilers for heating and power purposes. The Juggernaut of civilization slays at grade crossings in New Jersey, an average of a human being every forty-eight hours. Yet none of us would favor the abolition of the locomotive. Our country owes its prosperity to improved transportation. The modern steam railroad, however, perfect as we may consider it, does not supply all our needs. It must be supplemented by the horse car, the stage, the private vehicle and even the bicycle. Although our land seems gridironed with glistening rails, yet there are,

even in our most thickly settled states, large tracts of territory that are practically without adequate facilities for transportation. This is a field which we have reason to believe will soon be occupied by the electric railway. The progress already made has proved, that with proper engineering such a road can be operated with safety, efficiency and economy. The increased speed and superior service are so apparent, that its advantages to the public have already neutralized the prejudice existing against overhead wires, in the city of Boston. The result will soon be, that instead of rapid transit being confined to four main thoroughfares as is now the case in New York city, almost the entire street railway system will be operated at a higher speed than prevails on the New York elevated railroads to-day, and without the disadvantage of requiring passengers to ascend and descend long flights of stairs. Instead of facilitating improvements of this character, however, we find that our largest cities are adopting a policy of repression rather than regulation, the natural consequence of which is that those cities of secondary importance are in this respect taking the lead, the city of Scranton, for instance, having already entirely abolished animal-power and substituted the electric motor on all its lines.

The transmission and distribution of power by electricity for industrial purposes is, however, an exceedingly important field in which the electric motor is rapidly winning favor. It is natural that it should first be utilized for the service where its advantages are of the greatest importance and most attractive to the capitalist, and its adoption for street railway purposes has been followed very closely by its introduction in mining work, where its development has been retarded by attention being concentrated to other fields, rather than through any lack of appreciation on the part of mine owners. One of the most notable installations of this character is that at the Nevada mill, Virginia City, Nev., where the waste water from the turbine wheels at the surface is led through a vertical shaft 1,630 feet to a subterranean chamber driving six Pelton water wheels attached directly to the shafts of six dynamos, which are driven at



the rate of 900 revolutions per minute. The electric current thus generated is led back to the surface, and by means of six electric motors, the energy formerly wasted or utilized to little purpose, is made to perform service at the mill equal to 450 horse-power. So tremendous is the pressure of water at 1,630 feet head that the stream of water, as it issues from the nozzle, resists the blow of a hammer as in striking upon a bar of iron or steel. The waste water from the wheels is led off through the Sutro tunnel. It will be seen that in this case one of the most important advantages of electrical transmission is that the copper conductor can be carried through a shaft, utilizing but little space and making any desired bends or angles. It is, of course, important that the wires should be well insulated and located with proper care to avoid mechanical injury. A similar service has also been performed with rope transmission, but it is apparent to all who have given the subject attention, that the advantages of electricity will eventually be fully recognized. Indeed, it is to-day merely a matter of comparative cost. With the reduction of the manufacturing cost of motors, the lower cost of copper when artificial conditions are removed which now enhance its price, the use of high-pressure currents, where necessary, are all factors in the future utilization of electricity, which may be looked for with reasonable certainty within a very few years. It is in the distribution of power for small users, however, that the great public is most immediately interested. Almost every household may utilize light motors for various purposes. The only obstacle existing to-day to the installation of thousands of such motors, for fans, sewing-machines, pumps and similar work, is the difficulty of obtaining the electric current. Whether the possibility of obtaining power for light manufacturing will lead to the re-establishment of the little shops of early days, as has been predicted, appears somewhat problematical. The tendency of the times toward the concentration of craftsmen in large shops, is not because, altogether, of the difficulty of obtaining power, but is due rather to various well-known economies in manufacturing on a large scale, which have been

forced by the stress of competition. We should look upon electricity, not as in itself a force, but as the means of conveying that force for our benefit to any required point. There can be no more faithful agent. It is our own fault if we fail to provide the proper path over which it is to travel in our service. We know its requirements, and its laws are perfectly understood. Water, steam, gas, in fact, all our useful and familiar applications of nature's forces, must be treated in accordance with well-known laws, else in their might they will destroy us. We have become so familiar by long usage with the manifestations of nature's power, that we can scarcely realize the awe with which they must have been looked upon by the primeval inhabitants of the earth. One of the great thinkers of the day, Henry George, in arguing upon his favorite topic, "The Land Question," says:

"When, lit by lightning flash or friction among dry grasses, the consuming flames of fire first flung their lurid glow into the face of man, how he must have started back in affright! When he first stood by the shores of the sea, how its waves must have said to him, 'thus far shalt thou go, but no farther!' Yet, as he learned to use them, fire became his most useful servant, the sea his easiest highway. The most destructive element of which we know—that which for ages and ages seemed the very thunderbolt of the angry gods—is, as we are now beginning to learn, fraught for us with untold powers of usefulness. Already it enables us to annihilate space in our messages, to illuminate the night with new suns, and its uses are only beginning."

But what of the future? This is the era of prophecies as to the future of electricity, and many active minds are striving for the fortune that awaits the inventor of an economical process, for the production of electricity from coal without the wasteful intervention of steam. But even then, the crudity of the process does not commend itself, as the final solution of this great problem. If in the coal is stored the energy of the sun as expended on this planet thousands of years ago, why should we go on for years,

patiently blasting and digging out this wonderful mineral, slowly hauling it, hoisting it, screening it, bringing it again hundreds of miles by railway, and finally wasting a large share of its stored energy in our efforts to produce heat by combustion? After all this wasteful expenditure of energy in bringing this coal to our door, the best we can do to-day is to utilize from ten to fifteen per cent. of the force which has been secreted by nature for our use. Assuming as we must that the supply of coal will eventually be exhausted, our efforts would more properly be directed towards the utilization of force as supplied by nature at first hands. Whatever may be the financial outcome of our efforts in penetrating nature's secrets, we cannot go far astray if we look upon electricity as the agency of power. It appears to be nature's tool for placing her forces at our disposal. The very fact that it delivers energy instantaneously, in either of its practical forms of light, heat, or power, points out to us the perfect manner in which it has been adapted to our use. Science is not yet satisfied, however, with the conditions under which the light is now given to us. Nothing less than perfect light without heat or combustion—the secret of the phosphorescent rays of the glow-worm—will meet the advanced ideas of our modern investigators. Such a light may be perfected without the use of electricity, unless we accept the modern dictum, and which grows stronger as evidence accumulates, that light and electricity are the same. The distribution of heat through the agency of electricity, although not yet before the public as a practical system, is receiving much attention at the hands of ingenious inventors, who, it is evident, consider the subject of sufficient promise to warrant the devotion to it of time and money. To-day the most important applications of heat from the electric current are in the welding of metals, and the extraction of the wonderful metal aluminium through the medium of the electric furnace, both of which processes have already been brought to your attention in previous lectures, and I desire here to congratulate the worthy managers of this institute, its members and the community, for their successful efforts in encouraging progress, and

to thank them for the opportunities thus afforded for making known to the world the most recent developments in science and the arts for the benefit of the human family.

Although we marvel at the rapid strides recently made in electric development, we are inclined to be impatient at opposition encountered in the efforts made to extend the introduction of electric light and power. We should content ourselves with the reflection that this is but the ever-present inertia, which must be overcome before any stationary body can be moved. There exists in every community a conservative element, which has manifested itself in the past, and being a matter of record the present generation can well afford to laugh at the fears of our ancestors. I will cite a few instances which will do much toward reconciling us to the views of those who are to-day skeptical as to the benefits of electricity, or are sounding the alarm as to the dangers which must necessarily attend its general use.

In an article, entitled "How our Ancestors Travelled," we find the following pertinent observations on the subject:

"Carriages met with great opposition at their first introduction, and laws were made to suppress their use. As early as the year 1294, Philip the Fair, of France, issued an ordinance for suppressing luxury, in which the wives of citizens were forbidden the use of carriages. Beckmann tells us that there is preserved in the archives of the County of Mark, an edict, in which the feudal nobility and vassals are prohibited from using coaches under pain of incurring the punishment of felony. Duke John, of Brunswick, published an order in 1588, roundly rating his vassals for neglect of horsemanship, and forbidding them to appear or travel in coaches. A few years after this, the English Parliament took up the discussion of the subject; but, on the 7th of November, 1601, the bill to restrain the excessive use of coaches within the realm of England was rejected." But the bitterness of antagonism to them did not cease with this legislative decision. In a pamphlet, called the "Great Concern of England Explained," published 1673, the writer very gravely attempts to make out that the introduction of coaches was ruining the trade of the realm.



Following is an example of his method of reasoning: "Before coaches were set up, travellers rode on horseback, and men had boots, spurs, saddles, bridles, saddle-cloths, and good riding-suits, coats and cloaks, stockings and hats, whereby the wood and leather of the kingdom were consumed. Besides, most gentlemen, when they travelled on horseback, used to ride with swords, belts, pistols, holsters, portmanteaus and hat cases, for which in these coaches they have little or no occasion. For when they rode on horseback, they rode in one suit, and carried another to wear when they came to their journey's end; but in coaches they ride in a silk suit, silk stockings, beaver hats, etc., and carry no other with them. This is because they escape the wet and dirt, which upon horseback they cannot avoid; whereas, in two or three journeys on horseback these clothes and hats were wont to be spoiled; which done they were forced to have new very often, and that increased the consumption of manufactures." In another part of his pamphlet, the same writer puts the following query, evidently with the notion that it was a clincher: "Is it for a man's health or business to be laid fast in four ways; to ride all day with strangers, oftentimes sick, diseased, ancient persons, or young children crying; all whose humors he is obliged to put up with, and crippled with their boxes and bundles?" As an additional objection against the introduction of coaches, the writer urges that they would discourage the breeding and lessen the value of horses.

The following passage occurs in a protest against the construction of railways, which is preserved in the archives of the Nurnberg Railway at Furth, which was the first line constructed in Germany. It was drawn up by the Royal College of Bavarian Doctors:

"Travel in carriages drawn by a locomotive ought to be forbidden in the interest of public health. The rapid movement cannot fail to produce among the passengers the mental affection known as *delirium furiosum*. Even if travellers are willing to incur this risk, the Government should at least protect the public. A single glance at a locomotive passing rapidly is sufficient to cause the same cerebral

derangement, consequently, it is absolutely necessary to build a fence ten feet in height on each side of the railway."

These were all European, however, so in order to assure you that these peculiar views were held in our country, the following protest from the good citizens of Philadelphia, in 1833, against the introduction of gas, will be of interest to you :

PHILADELPHIA, November 28, 1833.

REMONSTRANCE AGAINST LIGHTING WITH GAS.

*To the Honorable, the Select and Common Councils of the City of Philadelphia :*

GENTLEMEN:—The subscribers beg leave to respectfully remonstrate against the plan now in action for *lighting the city with gas*, as they consider it a most offensive, inexpedient and dangerous mode of lighting. In saying this they are fully sustained by the accounts of explosions, loss of life and destruction of property where this mode of lighting has been adopted.

We consider gas to be as combustible as gunpowder and nearly as fatal in its acts; as regards the immense destruction of property, we believe that the vast number of fires in New York and other cities may be in a great measure ascribed to this mode of lighting. The leakage of pipes and carelessness of stopping off the gas furnish almost daily instances of its destructive effects. And when we consider that this *powerful and destructive agent* must necessarily be left often to the care and attention of youth and domestics and careless people, we only wonder that the consequences are not more *appalling*. It is also an uncertain light, sometimes suddenly disappearing and leaving streets and houses in total darkness.

The waters of the Delaware and Schuylkill, now considered the most pure and salubrious in the world, as many long voyages have fully tested, must soon, we fear, experience the deterioration which has reduced the water of the Thames to the present impure state, for no reservoir will be able to contain such fetid drains from such an establish-



ment, and very soon the rivers must be their receptacle, to the destruction of the immense shoals of shad, herring and other fish with which they abound; the same cause must produce the same effect. Salmon, smelts and other fish, formerly caught in vast quantities in the Thames, have nearly all disappeared. The constant digging up of the streets, the circumstances of the gas pipes, which, at the intersection of each square, must come in contact with the water pipes, are difficulties and evils which we would anxiously avoid.

In conclusion, we earnestly solicit that the *lighting our city with oil may be continued.*

And your petitioners, etc.

Signed by 1,200 of the leading citizens of Philadelphia, whose names are attached hereto, such as Horace Binney, Hartman Kuhn, Jacob Ridgway, Paul Beck, Henry Pratt, Benjamin Chew (on whose farm the battle of Germantown was fought), John Sargeant, Charles Wharton, Richard Willing, Edward Pennington, Robert Vaux, Joshua Longstreth, Matthew Newkirk, and 1,200 others.

N. B.—The above are only part of the names, as many of the remonstrances have not yet come in.

It is not best that the exploitation of a new industry should be too rapid. Improvements are continually being brought out, which render the devices of yesterday entirely obsolete. What we ask is that the merits of electricity for the various purposes for which we claim it is adapted, should be thoroughly investigated, that the exaggerated statements as to its dangers be carefully analyzed, and I would especially recommend that every mechanic, every business manager, every man concerned in the development of great enterprises should devote a little of his time to the study of electricity. Opportunities may present themselves at any moment where the use of electricity will prove important and beneficial. It is a duty we owe to ourselves and the community to make the best possible use of the resources placed in our hands. This is the age of mechanical progress. Year by year the hand of man is being more and

more superseded by the machine. For fifty years we have witnessed the material progress of an era of steam and iron. We are entering upon an age of electricity and aluminium. Let us unite in welcoming these accessions to human welfare, that the glory of the twentieth century may even surpass that which will soon pass into history. The nineteenth century has witnessed the triumphant development of the great principle established by Alessandro Volta in 1800—that electricity had entered submissively into the control of man—and with the exercise of skill and intelligence in his control, it will remain until the dawn of the millennium, for it is the edict of nature that it shall prevail until the end.

I am indebted to the researches of my esteemed friend, Thomas D. Lockwood, for the dates of the following interesting events in electrical history :

1729.

July 3.—Electrical insulation discovered by Stephen Gray.

1747.

July 14.—Electricity passed through a wire across the Thames over Westminster bridge, the circuit being completed through the water.

1752.

July 4.—Benjamin Franklin demonstrated the identity between lightning and electricity by flying a kite during a thunder-storm.

1753.

Feb. 1.—First suggestion of the electric telegraph, in a communication signed C. M. in *Scott's Magazine*.

1800.

Mar. 20.—Alessandro Volta announced his invention of the voltaic pile.

April 30.—Nicholson and Carlisle discover the principle of electro-chemical deposition.

Sept. 22.—Spark from voltaic battery, as observed by Davy, announced in *Nicholson's Journal*.

Oct. 24.—First telegraph patent taken out in the United States by Jonathan Grout.

1820.

July 21.—Oersted's discovery of electro-magnetism first announced.

Sept. 15.—The invention of the galvanometer announced by Schweigger.

Sept. 25.—Arago announced before the French academy his discovery that a copper wire energized by electricity became magnetic.

1827.

Oct. 10.—Joseph Henry announces his invention of a practical electro-magnet.

1831.

Aug. 27.—An electric current from a permanent magnet developed by Faraday.

Oct. 1.—Faraday produced a current in one circuit by induction from a current flowing in another. First in Great Britain,

1832.

Mar. 30.—Magneto-electric spark first obtained from permanent magnet by Prof. Forbes, Edinburgh, Scotland. It appears that the document giving an account of the excitation of a spark from a permanent magnet, is by Signor Nobili, and another dated from the Museum at Florence, January 31, 1832.—*Noad, 1859.*

Oct. 19.—Morse first conceived his idea of an electro-magnetic telegraph on board the ship *Sully*, between Havre and New York.

Dec. 15.—Faraday used the earth to complete a voltaic circuit by means of water and gas pipes.

1836.

July 23.—Daniell announced the invention of his sulphate of copper constant battery.

June 24.—First patent in electro-metallurgy issued in England to G. R. Elkington.

Dec. 23.—Steinheil's telegraph first described in the *Magazine of Popular Science*.

1837.

June 6.—English patent for electromotor issued to Thomas Davenport, of Brandon, Vt.

July 25.—English needle telegraph first practically operated on a subterranean line between Easton and Camden Town.

Sept. 2.—Morse made his first experiment in telegraphy.

1839.

May 4.—The invention of electrotyping announced by Jacobi.

May 21.—Heat manifested by the transmission of electricity through suitable wires from the gymnotus or electric eel.

Nov. 2.—Electro-magnetic motor exhibited in England by Captain Taylor.

1840.

June 20.—Patent for electro-magnetic telegraph issued to Prof. Morse.

1841.

Aug. 21.—First patent describing the incandescent electric lamp issued in England to F. de Moleyns.

1842.

Aug. 1.—The use of the magneto-electric machine for electro-deposition suggested in a British patent taken out by J. S. Woolrich.

1844.

May 24.—First message sent over a telegraph wire by the Morse system.

1845.

April 1.—First American electric telegraph opened for business between Washington and Baltimore.

Nov. 4.—Incandescent carbon in a vacuum patented in England by Starr, in the name of E. A. King.

1846.

April 11.—Patent granted to Prof. Morse for his invention of the local circuit operated by a main line relay.

1849.

Jan. 10.—First experimental submarine cable laid by C. V. Walker, two miles out at sea from Folkestone, England.

1851.

Feb. 7.—Central office system patented by F. M. A. Dumont, in England.

April 29.—Prof. Charles G. Page makes an experimental trip in his electro-magnetic locomotive from Washington to Bladensburg and return, the highest speed being 19 miles per hour.

1858.

May 18.—First arc lamp patent issued in the United States.

Aug. 5.—The laying of the first Atlantic cable completed, which worked for 26 days.

Nov. 24.—Dynamo principle described in an English patent.

Dec. 8.—Electric light first employed in light-house work at South Foreland, England.

1859.

Sept. 20.—First patent granted in the United States for heating by electricity.

1866.

July 27.—Telegraph communication successfully and permanently established between Europe and America.

Dec. 24.—S. A. Varley provisionally patents in England the dynamo principle, first described and patented by Hjorth in 1854, and subsequently elaborated in an English patent of 1858.

1867.

Jan. 17.—Werner Siemens proposed the name dynamo machine in an address before the Berlin academy.

Feb. 14.—Sir Charles Wheatstone explains the dynamo principle before the Royal Society.

1873.

June 3.—The reversibility of the dynamo demonstrated by Hippolyte Fontaine.

1875.

June 2.—First form of the Bell telephone made.

1876.

Mar. 7.—Patent granted to Alexander Graham Bell for the telephone.

Mar. 23.—First patent granted to Jablochhoff for his electric candle.

1877.

April 4.—First telephone line completed between Boston and Somerville, Mass.

1880.

Jan. 27.—Patent granted to Thomas A. Edison for an electric incandescent lamp having a carbon filament of high resistance.

Oct. 20.—The mechanical formation of secondary battery plates patented in France by Camille Faure.

1886.

Aug. 10.—Electric welding patented by Prof. Elihu Thomson.

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## SOME PROPERTIES OF CONFOCAL ELLIPSES AND THEIR APPLICATION TO MECHANISM.

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BY HORACE B. GALE,  
Washington University, St. Louis.

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In several books on mechanism a graphical method is described, by which confocal ellipses are made use of in constructing the pitch outlines of sets of lobed wheels having various numbers of lobes. In Rankine's *Machinery and Millwork*, the discovery of this method is attributed to the Reverend W. Holditch. No demonstration of the rolling properties of these wheels is there given, however, nor has any demonstration ever been published within the writer's knowledge.

The following is offered as a contribution to the science of mechanism, and as bringing out certain properties of confocal ellipses which are not set forth in existing works on analytical or descriptive geometry.

In *Fig. 1* are represented two semi-ellipses having the same foci,  $F_1$  and  $F_2$ . About the common centre  $O$  are



drawn auxiliary circles, having radii equal respectively to the semi-major axes,  $OA_1, OA_2$ . Let  $OC_1C_2$  represent any radial line cutting the circles in points  $C_1$  and  $C_2$ , respectively. From any such pair of points let fall perpendiculars to the major axes,  $C_1D_1$  and  $C_2D_2$ , cutting the ellipses in a pair of points,  $P_1$  and  $P_2$ .

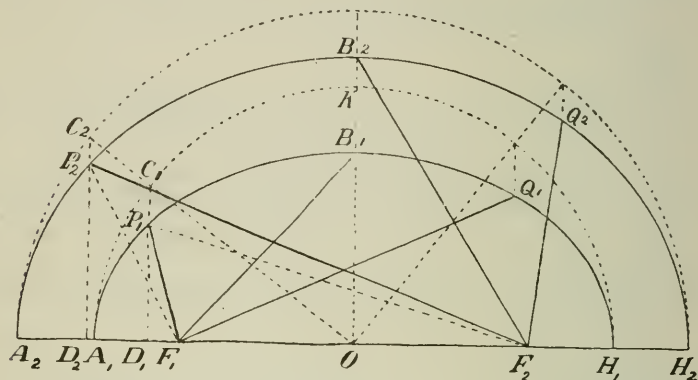


FIG. 1.

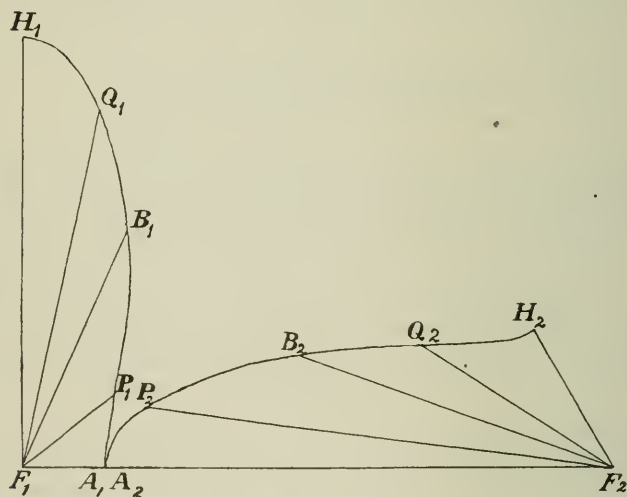


FIG. 2.

In *Fig. 2*, let  $F_1$  and  $F_2$  represent fixed centres about which a pair of sectors are to turn in rolling contact with each other, the distance  $F_1F_2$  being equal to the sum of the semi-

major axes of the ellipses ( $OA_1 + OA_2$ ); and let the focal distances of successive points on one of the ellipses, as  $F_1A_1$ ,  $F_1P_1$ ,  $F_1B_1$ , etc., be laid off in order on successive radial lines from the fixed centre  $F_1$ , while the focal distances of the corresponding points of the other ellipse,  $F_2A_2$ ,  $F_2P_2$ ,  $F_2B_2$ , etc., are similarly laid off from the centre  $F_2$ . Also let the angle between the focal lines drawn to each two successive points of the first ellipse, as the angles  $A_1F_1P_1$ ,  $P_1F_1B_1$ , etc., be reduced, or *contracted*, in *Fig. 2* in any fixed ratio,  $\frac{1}{n_1}$ ; and let the focal angles of the second ellipse, as  $A_2F_2P_2$ ,  $P_2F_2B_2$ , etc., be contracted in *Fig. 2* in the ratio  $\frac{1}{n_2}$ ; the numbers  $n_1$  and  $n_2$  being taken proportional to the lengths of the semi-minor axes of the corresponding ellipses,  $OB_1$  and  $OB_2$ .

The proposition now to be proved is, that two sectors, constructed in the manner described, are capable of turning in perfect rolling contact about the fixed points  $F_1$  and  $F_2$ , and that each pair of corresponding points of the two pitch lines, as  $P_1$  and  $P_2$ , will coincide, as they come into the line of centres.

In order that any two non-circular sectors may turn about fixed centres in perfect rolling contact, the two following conditions must be fulfilled: (1) for each pair of corresponding points, as  $P_1$  and  $P_2$ , the sum of the distances of the points from the respective centres,  $F_1P_1 + F_2P_2$ , must be equal to the distance between the centres; (2) at each point of either pitch line, as  $P_1$ , the *obliquity* must be the same as that of the other pitch line at the corresponding point,  $P_2$ . By the obliquity of the pitch line is meant the angle made by a tangent to the curve at a given point, and a tangent to a circle about the fixed centre or focus, at the same point; for example, the angle  $MPN$ , in *Fig. 3*. The condition usually stated for perfect rolling contact—that the distances between corresponding points, measured along the two pitch lines, must be equal—is included, at least for non-circular figures, in the statement just made, that the pitch lines must have equal obliquity at each pair of corresponding points.

It is, therefore, to be proved that in the sectors constructed

as described, the obliquity of the pitch line at any point  $P_1$  (*Fig. 2*) is the same as the obliquity at the corresponding point  $P_2$ . Let us first refer to *Fig. 1*, and find the relation between the obliquity of the first ellipse at  $P_1$  and that of the other at  $P_2$ .

Let  $a_1$  and  $a_2$  represent the semi-major axes,  $b_1$  and  $b_2$  the semi-minor axes, and  $e_1$  and  $e_2$  the eccentricities, of the two ellipses, respectively. Let  $o_1$  represent one-half the angle  $F_1P_1F_2$ , and  $o_2$  one-half the angle  $F_1P_2F_2$ . Then  $o_1$ ,  $o_2$ —being each equal to the angle between a normal to the ellipse and a focal line—are respectively equal to the angles of obliquity of the two ellipses at  $P_1$  and  $P_2$ . Let the centre  $O$  be taken as the origin,  $x_1$  and  $y_1$  as the coördinates of  $P_1$ , and  $x_2$  and  $y_2$  as the coördinates of  $P_2$ .

In analytical geometry (for example, see Todhunter's *Conic Sections*, p. 156, art. 178), is deduced the following general value for the tangent of the angle made by the normal and focal line at any point of an ellipse :

$$\tan o = \frac{e a y}{b^2}.$$

Applying this equation to the ellipses of *Fig. 1*, we have

$$\frac{\tan o_1}{\tan o_2} = \frac{e_1 a_1 y_1}{b_1^2} \cdot \frac{b_2^2}{e_2 a_2 y_2}. \quad (1)$$

The eccentricity of an ellipse is the ratio of one-half the distance between the foci to the semi-major axis; therefore

$$e_1 = \frac{OF_1}{a_1},$$

$$e_2 = \frac{OF_1}{a_2},$$

and

$$e_1 a_1 = e_2 a_2 = OF_1. \quad (2)$$

We have also, from *Fig. 1*,

$$\frac{C_1 D_1}{P_1 D_1} = \frac{OK}{OB_1}.$$

or

$$\frac{C_1 D_1}{y_1} = \frac{a_1}{b_1}. \quad (3)$$

Similarly,

$$\frac{C_2 D_2}{y_2} = \frac{a_2}{b_2}. \quad (4)$$

Dividing (4) by (3), we have

$$\frac{C_2 D_2}{y_2} \cdot \frac{y_1}{C_1 D_1} = \frac{a_2}{b_2} \cdot \frac{b_1}{a_1};$$

but

$$\frac{C_2 D_2}{C_1 D_1} = \frac{OC_2}{OC_1} = \frac{a_2}{a_1};$$

therefore

$$\frac{y_1}{y_2} = \frac{b_1}{b_2}. \quad (5)$$

Substituting values from equations (2) and (5) in equation (1), and reducing the expression to its lowest terms, we have, for any pair of corresponding points, as  $P_1$  and  $P_2$ , in confocal ellipses,

$$\frac{\tan \phi_1}{\tan \phi_2} = \frac{b_2}{b_1}. \quad (6)$$

This equation expresses the relation between the tangents of the angles of obliquity of the two ellipses at  $P_1$  and  $P_2$ . It remains to determine the effect of contracting the angles between the focal lines, as is done in constructing the sectors in *Fig. 2*.

When the focal angles of any curve are contracted in a given proportion, the length of the radii remaining constant, the ratio of the angle between any two radii to the difference in length of those radii is reduced in the given proportion. The value of the differential coefficient,

$$\frac{d\theta}{dr},$$

at any point in the contracted curve, is accordingly less





and  $n_2$  were taken proportional to the lengths of the semi-minor axes, we have

$$\frac{n_2}{n_1} = \frac{b_2}{b_1};$$

and substituting this value for  $\frac{b_2}{b_1}$  in equation (6), we obtain

$$\frac{\tan o_1}{\tan o_2} = \frac{n_2}{n_1};$$

or,

$$n_1 \tan o_1 = n_2 \tan o_2.$$

Therefore, the obliquity of the pitch line of one of the sectors at any point  $P_1$  is equal to the obliquity of that of the other at the corresponding point  $P_2$ .

If now it can be proved also that at every pair of corresponding points, as  $P_1$  and  $P_2$ , the sum of the distances  $F_1P_1$  and  $F_2P_2$  is equal to the distance between the two fixed centres, or if, for every such pair of points on the ellipses,

$$F_1P_1 + F_2P_2 = a_1 + a_2,$$

then we know that the two sectors will work together in perfect rolling contact. In *Fig. 1*, we have

$$\overline{P_1F_1}^2 = (x_1 - OF_1)^2 + y_1^2, \quad (7)$$

$$\overline{P_2F_2}^2 = (x_2 + OF_1)^2 + y_2^2. \quad (8)$$

From (7),

$$\overline{P_1F_1}^2 = x_1^2 - 2 \overline{OF_1} x_1 + \overline{OF_1}^2 + y_1^2. \quad (9)$$

Applying the equation of the ellipse, we have

$$\frac{x_1^2}{a_1^2} + \frac{y_1^2}{b_1^2} = 1;$$

or

$$y_1^2 = b_1^2 - \frac{b_1^2}{a_1^2} x_1^2.$$

Substituting this value for  $y_1^2$  in (9), we have

$$\overline{P_1F_1}^2 = x_1^2 - 2 \overline{OF_1} x_1 + \overline{OF_1}^2 + b_1^2 - \frac{b_1^2}{a_1^2} x_1^2;$$

or

$$\overline{P_1 F_1^2} = \frac{a_1^2 - b_1^2}{a_1^2} x_1^2 - 2 \overline{OF_1} x_1 + \overline{OF_1^2} + b_1^2.$$

But

$$a_1^2 - b_1^2 = \overline{OF_1^2};$$

therefore

$$\overline{P_1 F_1^2} = \frac{\overline{OF_1^2}}{a_1^2} x_1^2 - 2 \overline{OF_1} x_1 + a_1^2 - b_1^2 + b_1^2;$$

or

$$P_1 F_1 = a_1 - \frac{OF_1}{a_1} x_1. \quad (10)$$

By a similar process we obtain from equation (8)

$$P_2 F_2 = a_2 + \frac{OF_1}{a_2} x_2. \quad (11)$$

Adding equations (10) and (11), we have

$$P_1 F_1 + P_2 F_2 = a_1 + a_2 + \left( \frac{x_2}{a_2} - \frac{x_1}{a_1} \right) OF_1.$$

But, from *Fig. 1*,

$$\frac{x_1}{a_1} = \frac{x_2}{a_2};$$

therefore

$$P_1 F_1 + P_2 F_2 = a_1 + a_2, \quad Q. E. D.$$

The principles which have now been proved may readily be applied to the construction of the pitch lines of a set of interchangeable lobed wheels, having various numbers of lobes, any one of which is capable of working in rolling contact with any other, or with a given ellipse, which is itself a one-lobed wheel. *Fig. 5* shows the pitch lines of a pair of such wheels, having two and three lobes, respectively.

The angle included between the shortest and the longest radius of a lobe of a symmetrical wheel, whose whole num-

ber of lobes is  $n$  (for example, the angle  $DAC$ , in *Fig. 5*), must be

$$\frac{360^\circ}{2n}, \text{ or } \frac{180^\circ}{n}.$$

Now, if each half lobe is derived by the process of contracting the angles between the focal lines of a semi-ellipse—the angle between the shortest and the longest of those lines in the ellipse being  $180^\circ$ —the ratio in which the angles must be contracted for a wheel of  $n$  lobes will be evidently  $\frac{1}{n}$ . In other words, the ratio of contraction of the angles of the base ellipse, used in deriving the pitch line of a lobe of any wheel of the set, will be one divided by the number of lobes; and the ratios of contraction for different wheels of the set will be to each other inversely as their numbers of lobes. But in order that the various lobes may work correctly together, the obliquity at corresponding points of their pitch lines must be the same; which, as has been shown, is true only when the ratios of contraction are inversely as the semi-minor axes of the base ellipses. It follows that the minor axes of the base ellipses for a set of interchangeable lobed wheels must be proportional to the numbers of lobes of the corresponding wheels. The focal angles are then to be contracted to one-half the original value for a two-lobed wheel, one-third for a three-lobed wheel, and so on. For a one-lobed wheel, the ratio of contraction would be unity; in other words, the one-lobed wheel will be itself an ellipse equal to its base ellipse, arranged to turn about one of its foci.

The base ellipses for all the wheels of a set must therefore be constructed on the same foci as the ellipse forming the one-lobed wheel, and with minor axes equal respectively to the minor axis of the one-lobed wheel, multiplied by the number of lobes. The outlines of the lobes of the various wheels are then constructed by contracting the angles of each base ellipse in the ratio of the reciprocal of the number of lobes. Any wheel of such a set will work correctly with any other, or with another wheel like itself. It follows that any ellipse will work in rolling contact with an equal

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ellipse, when each is made to turn about one of its foci, the distance between the centres of rotation being equal to the major axis.

It is evident, also, that by making use of any part of the arc of a single ellipse (as  $A_2P_2$ , in *Fig. 1*) subtending different

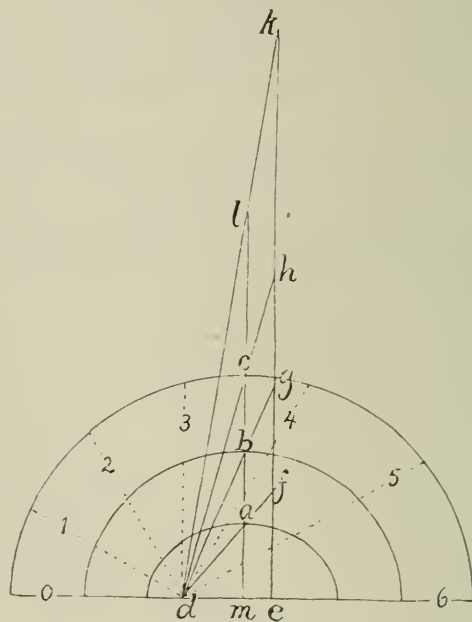


FIG. 4.

angles at the two foci, and contracting or expanding these angles in the *same* ratio, pairs of sectors may be derived which will work correctly with each other, and turn through any desired angles. Moreover, if the angles subtended by these sectors are made any aliquot parts of  $180^\circ$ , they may be used as the half lobes of symmetrical lobed wheels. Again, by combining about a common centre sectors derived from elliptical arcs having different foci, each working with a sector derived from the same arc, or from a corresponding arc of a confocal ellipse, an infinite variety of non-symmetrical lobed wheels may be formed. Such lobed wheels may also be formed by combining arcs derived from ellipses with

logarithmic spirals, or suitable irregular curves, each paired with a suitable corresponding arc.

A number of special methods of applying these general principles are well set forth in Prof. C. W. MacCord's treatise

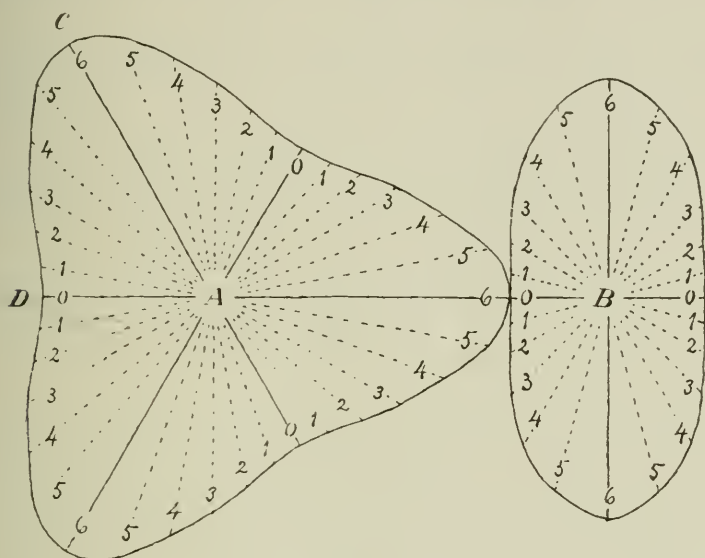


FIG. 5.

upon the *Kinematics of Mechanical Movements*, Part I. Other special problems of interest are treated in Rankine's *Machinery and Mill Work*. One other point will be noticed in this article, viz.: the formation of non-circular wheels whose outlines are to have a *definite maximum obliquity*.

The maximum obliquity of the pitch line of any one of a set of interchangeable lobed wheels must be the same as that of the one-lobed wheel belonging to the set; and the maximum obliquity of an ellipse—corresponding to the maximum value of the angle  $\phi$ , in *Fig. 1*—will be at a point in the curve situated at the end of the minor axis. The obliquity at that point will be equal to the angle between the minor axis and a focal line. The following is given as an example of a special application of these principles.

Let it be required to construct the pitch lines for a two-lobed wheel and a three-lobed wheel to turn in rolling con-



tact about the centres  $A$  and  $B$ , situated at a fixed distance apart, the maximum obliquity of the pitch lines to be  $40^\circ$ . In *Fig. 4*, first lay off the right angle  $def$ , and from any convenient point  $d$  draw  $df$ , making  $edf = 50^\circ$  (the complement of the desired maximum obliquity). Take  $fg$  and  $gh$ , each equal to  $ef$ , and make  $dk = dg + dh$ . On  $dk$ , take  $dl$  equal to the required distance between the centres  $A$  and  $B$ . Draw  $lm$  parallel to  $ke$ ; then  $db + dc = dl = AB$ ; therefore  $mb$  and  $mc$  are the semi-minor axes, and  $d$  a focus of the required base ellipses, which may now be constructed. The focal angles of the larger ellipse are contracted to one-third their value in forming a lobe of the three-lobed wheel, and those of the smaller ellipse to one-half their value in forming the two-lobed wheel. The radii in *Fig. 5* are numbered to correspond with the focal lines in *Fig. 4*. The ellipse, whose minor axis is  $ma$ , is the outline of a one-lobed wheel belonging to the same set.

By using the same set of focal lines for all the base ellipses, as is done in *Fig. 4*, the process is somewhat simplified over that used in *Fig. 1*, which is necessary only when *corresponding points* in the various pitch lines are to be found. The method of deriving the lobed wheels from the ellipses, illustrated by *Figs. 4* and *5*, is the one which is usually given in books on mechanism, but of which no demonstration has heretofore been published. That part of the construction of *Fig. 4*, fixing the maximum obliquity and the distance between the centres of the lobed wheels, and the method of finding corresponding points, illustrated by *Figs. 1* and *2*, are believed to be now published for the first time.

## HIGH EXPLOSIVES IN WARFARE.

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BY COMMANDER F. M. BARBER, U. S. N.

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*[A lecture delivered before the Franklin Institute, November 28, 1890.]*

MEMBERS OF THE INSTITUTE AND LADIES AND GENTLEMEN:

In commencing my paper this evening I desire to call your attention to the fact that I am dealing with a subject which, though not theoretical, is still hardly practical, for as a matter of fact, high explosives cannot be said to have yet been regularly used in warfare, and I hope you will pardon me if in consequence, my statements appear in some respects unsatisfactory and my theories unsound. My subject, however, is no more obscure than future naval warfare generally. All civilized nations are spending millions of money for fighting purposes directly in opposition to the higher feelings of the better class of their inhabitants. The political atmosphere of Europe is the cause of this, but its consequence is the development of theoretical plans of ships which are no sooner commenced than the rapid march of mechanical, chemical and electrical science shows them to be faulty in some particular feature, and others are laid down only to be superseded in their turn.

None of these crafts are obsolete (to use the popular expression of the day). All are theoretically better than any which have stood the test of battle; but each excels its predecessor in some particular feature. The use of high explosives is the direct cause of the very latest transformations in marine architecture, and is destined to work still greater changes; but it will require a war between the most civilized nations of the world, and a long war to either confirm or condemn the many theoretical machines and methods of destruction that modern science has produced. I say a war between the most civilized nations, since it is only they that can supply the educated intellect that is necessary to both attack and defence. Under other circumstances false conclusions as to

weapons and results are certain to be drawn. At the bombardment of Alexandria, the English armor-clads, with their rifled guns, were not nearly as efficient against the feeble chalk fortifications as our wooden ships would have been with smooth bore guns; on the other hand I saw on shore after the bombardment hundreds of torpedoes and miles of cable that the Egyptians did not understand how to use. The French war with China was equally unsatisfactory from a military point of view. The Chinese at Foochow were annihilated because the French opened fire first, and the only shell that penetrated a French iron-clad was filled with lampblack instead of powder. The national riots that we are accustomed to hear of in South America are likewise of little instructive value; they buy their weapons of more civilized people, but there is always something fatally defective about the tactics pursued in using them. It may be said in general terms that in these days of extreme power in fighting machines, the greater the efficiency the less the simplicity and the more knowledge required in the care of the weapons. When powder was merely powder the advice of the old adage to "trust in God and keep your powder dry" was ample to maintain the efficiency of the powder for all purposes; but nowadays, if you keep your powder dry you will burst your gun, and if you keep your gun-cotton dry you are liable to blow up your ship.

It is rather difficult to-day to define what high explosives are, in contradistinction to gunpowder. Thirty years ago we could say that powder was a mechanical mixture and the others were chemical compounds; but of late years this difference has disappeared.

The dynamical difference, however, still remains; gunpowder in its most efficient form is a slow-burning composition, which exerts a relatively low pressure and continues it for a long time and to a great distance; high explosives on the contrary, in their most efficient form, are extremely quick-burning substances, which exert an enormous pressure within a limited radius. Ordinary black gunpowder consists of a mechanical mixture of seventy-five per cent. of saltpetre, fifteen per cent. of charcoal and ten per cent.

of sulphur. The most important of the high explosives are formed by the action of nitric acid upon organic substances or other hydrocarbons, the compound radical  $\text{NO}_2$  being substituted for a portion of the hydrogen in the substance. The bodies thus formed are in a condition of unstable equilibrium; but if well made from good material, they become stable in their instability, very much like Prince Rupert's drops, those little glass pellets which endure almost any amount of rough usage; but once cracked, fly into infinitesimal fragments.

The power exerted by these nitro substitution products is due to the fact that they detonate, *i. e.*, they are instantaneously converted into colorless gas at a very high temperature, and in addition, they have almost no solid residue, Nitro-glycerine actually leaves none at all, while gunpowder leaves sixty-eight per cent. The first departure in gunpowder from the old-time constituents of black powder just mentioned, was for the purpose of obtaining less pressure and slower combustion than could be produced by mere granulating or caking; this was accomplished by using underburned charcoal together with sugar and about one and one-half per cent. of water. This is the brown powder most generally used at present and with satisfactory results; but the abstraction of its moisture increases its rapidity of combustion to a dangerous degree, besides which the underburned charcoal is itself unstable.

The next change demanded is smokelessness and to accomplish it, recourse is had to the high explosive field, mechanically mixing various substances with them to reduce and regulate their rapidity of action. Just now some form of gun-cotton is most in use mixed with nitrate of ammonia, camphor and other articles. The tendency of these mixtures is to absorb moisture, and the gun-cotton in them to decompose, and there is no smokeless powder which can to-day be considered successful. Such a powder, however, will undoubtedly be an accomplished fact in the near future. Military men seem to be a great deal at variance as to its value in the field but there can be no doubt of its value for naval purposes; it is a necessity

forced upon us by the development of torpedo warfare. First came the simple torpedo, at the end of an ordinary boat's spar; then came the special torpedo boat with its great speed; then the revolving cannon and rapid fire gun to meet the torpedo boat. At present the possible rapidity of fire is much greater than can be utilized on account of the smoke; hence the necessity of smokeless powder. Smokelessness is, however, principally a martial demand that has been made upon the science of explosives and has attracted public attention on that account. The commercial demands for various other properties has been much greater than the military, and between gunpowder near one end of the line in point of power and nitro-glycerine near the other, there are now over 350 different explosives manufactured, and most of these have been invented within the last twenty years.

The simplest application of high explosives in warfare is in connection with torpedoes, since within the same bulk a much more efficient substance can be obtained than gunpowder and with reasonable care there is very little danger of premature explosions by reason of accidental shocks.

Torpedoes were made by the Chinese many years ago; they were tried in our war of independence, and also by the Russians during the Crimean war; but the first practical and successful use of them as a recognized weapon was during our war of secession, when thirty-seven vessels were either sunk or seriously injured by them. Gunpowder was used in these torpedoes, though it is stated that attempts were made to use other substances without success. Since that time all maritime nations have made a close study of the subject and have adopted various high explosives, according to the results of their experiments. In general terms it may be stated that explosive chemical compounds have been found more suitable than explosive mixtures, because of the uniformity of direction in which they exert their pressure, and from the fact that water does not injure them. Mixtures may be very powerful but they are erratic and require tight cases. In the United States we use dynamite for harbor mines. It is composed of seventy-five



per cent. nitro-glycerine and twenty-five per cent. silica; but blasting gelatine and forcite gelatine will probably be adopted, when they can be satisfactorily manufactured here, as they are more powerful. The former is composed of ninety-two per cent. of nitro-glycerine and eight per cent. of gun-cotton, and the latter of ninety-five per cent. of nitro-gelatine and 5 per cent. unnitrated cellulose.

For naval use we have adopted gun-cotton as being the most convenient. In Europe gun-cotton is generally used for both fixed mines and movable torpedoes; Russia, Austria and Italy use blasting gelatine also.

In actual warfare but little experience has been had; two Peruvian vessels were sunk by dynamite in the Chile-Peruvian war; one Turk by means of gun-cotton during the Turco-Russian war of 1877, and two Chinese by gun-cotton in the Franco-Chinese war of 1884.

In making experiments to determine the relative strength of the different explosives under water, very curious and puzzling results have been obtained. Nitro-glycerine being the simplest and most complete in its chemical decomposition, and apparently the most powerful in air, it was natural to suppose that it would be the same in submarine work; but it was found by Gen. Abbot at Willet's Point after repeated experiments, as shown in his report of 1881, that it was not so powerful in its effect by twenty per cent. as dynamite No. 1, although the dynamite contained twenty-five per cent. of an absolutely inert substance. His idea was that it was too quick in its action, and since water is slightly compressible, a minute fraction of time is required in the development of the full force of the explosive. Gen. Abbot's results for intensity of action per unit of weight of the most important substances, is as follows:

Blasting gelatine, . . . . .	142
Forcite gelatine, . . . . .	133
Dynamite No. 1, . . . . .	100
Gun-cotton, wet, . . . . .	87
Nitro-glycerine, . . . . .	81
Gunpowder, . . . . .	20 to 50

Col. Bucknill, of the Royal Engineers, in his publication of 1888, gives the following :

Blasting gelatine, . . . . .	142
Forcite gelatine, . . . . .	133
Dynamite No. 1, . . . . .	100
Gun-cotton, dry, . . . . .	100
Gun-cotton, wet, . . . . .	80
Gunpowder, . . . . .	25

In both tables dynamite No. 1 is assumed as the standard of comparison. Col. Bucknill states that his gun-cotton results differ from Gen. Abbot's, because he experimented with much larger quantities, viz: 500-pound charges. Gen. Abbot's experiments led him to believe that an instantaneous mean pressure of 6,500 pounds per square inch would give a fatal blow to the double bottom of a modern armor-clad, and he developed a formula which gives this blow with blasting gelatine at the following distances under water, viz:

	<i>Pounds.</i>
At 5 feet, . . . . .	4
10 " . . . . .	17
20 " . . . . .	67
30 " . . . . .	160
40 " . . . . .	311

Col. Bucknill's experiments caused him to believe that a pressure of 12,000 pounds per square inch is required, and his formula, which is somewhat different from Abbot's, gives widely different results at close quarters, but they approach each other as the distance increases.

His results are as follows:

	<i>Pounds.</i>
At 5 feet, . . . . .	23½
10 " . . . . .	75
20 " . . . . .	177
30 " . . . . .	274
40 " . . . . .	369

Regarding the comparative effects of gunpowder and the high explosives, I think Gen. Abbot's estimate of a varying value for powder is more admissible than the fixed value assigned by Col. Bucknill. Gunpowder gives a

push and detonating compounds a shock; as the quantities increase, the push reaches farther than the shock. According to Gen. Abbot 100 pounds of dynamite No. 1 will have a destructive horizontal range of 16·3 feet, while the same amount of gunpowder will only have a range of 3·3 feet. Five hundred pounds of dynamite, however, will have a horizontal range of thirty-five feet, and 500 pounds of gunpowder will have 19·5 feet; the ratio has diminished from five to two. Whether 6,500 pounds or 12,000 pounds per square inch is necessary to crush the bottom of an armor-clad will depend largely upon how far apart the frames of the ship are spaced and what other bracing is supplied, as well as many local circumstances. It is difficult to judge exactly of these matters. Some four years ago, the Italian Government adopted treble bottoms for their heaviest ships as a result of experiments with seventy-five pounds of gun-cotton (the charge of an ordinary Whitehead locomotive torpedo) against a caisson which was a fac-simile of a portion of the proposed ships. Only two of the bottoms were broken through, and when the space between the two inner bottoms was filled with coal, only the outer bottom was broken. According to the formulæ of either Abbot or Bucknill, there should have been a local pressure of at least 300,000 pounds per square inch on the outer skin, and yet judicious interior arrangements rendered it harmless to the target. It would not, however, be safe to conclude that the torpedo was thus vanquished—the immediate result was simply to create a demand for larger locomotive torpedoes for local application, and but little light was thrown upon the results which might be anticipated from a large mine at a greater distance, whose radius of explosive effect would embrace a larger portion of the ship, and especially if the ship were nearly over the torpedo. The local effect of a detonation is different from the transmitted shock. Experiments in England have shown that 500 pounds of gun-cotton at forty feet below any ship will sink her, and at a horizontal distance of 100 feet, damage to the interior pipes and machinery is to be expected.

The fact that the high explosives are so much heavier

than gunpowder has an important bearing on the size of the containing case. Their sp. gr. is as follows :

Nitro-glycerine, . . . . .	1'6
Blasting gelatine, . . . . .	1'45
Forcite gelatine, . . . . .	1'51
Dynamite No. 1, . . . . .	1'6
Wet gun-cotton, . . . . .	1'32
Dry gun-cotton, . . . . .	1'06
Gunpowder, . . . . .	0'9

Their relative efficiency under water per cubic foot, according to Bucknill, is as follows :

Blasting gelatine, . . . . .	1'38
Forcite gelatine, . . . . .	1'27
Dynamite No. 1, . . . . .	1'00
Dry gun-cotton, . . . . .	'66
Wet gun-cotton, . . . . .	'66
Gunpowder, . . . . .	'14

The wet gun-cotton has twenty-five per cent. of added water.

Mines for harbor defence are of two kinds—buoyant and ground. The buoyant are usually spherical, and contain from 400 to 500 pounds of explosive. They bring the charge near to the ship's bottom, but are difficult to manage in a tide-way, and can be easily found by dragging. The ground mines can be made of any size and are not easily found by dragging, but are of little value in very deep water. They are either cylindrical, or hemispherical in shape, and contain from 500 to 1,500 pounds of explosive in from thirty to eighty feet of water. Mines of any kind are exceedingly difficult to render efficient when the water is over 100 feet deep. On account of the tendency of all high explosives to detonate by influence or sympathy and the liability of the cases to collapse by great exterior pressure, harbor mines are separated a certain distance, according as they are buoyant or ground, and according to the nature of the explosive.

Five hundred pounds buoyant gun-cotton mines require 320 feet spacing.

Five hundred pounds buoyant blasting gelatine mines require 450 feet spacing.

Six hundred pounds ground gun-cotton mines require 180 feet spacing.

Six hundred pounds ground blasting gelatine mines require 230 feet spacing.

Of torpedoes, other than those described, we have several modern varieties; submarine projectiles, submarine rockets, automobile and controllable locomotive torpedoes. The two first varieties, though feasible, are not developed and have not yet advanced beyond the experimental stage. Of the automobile, we have the Whitehead, Swartzkopf and Howell. The first two are propelled by means of compressed air and an engine; the last by the stored-up energy of a heavy fly-wheel. Generally speaking, they are cigar-shaped crafts, from 10 to 18 feet long and 15 to 17 inches in diameter, capable of carrying from 75 to 250 pounds of explosive at a rate of 25 to 30 knots for 400 yards, at any depth at which they may be set. Of the controllable locomotive torpedoes, the three representative types are the Patrick, Sims and Brennan. They are in general terms cigar boats, about 40 feet long and 2 feet in diameter, carrying charges of 400 pounds of explosive. The Patrick and Sims are maintained at a constant depth under water by means of a float. The Brennan has diving rudders like a Whitehead or a Howell. The Patrick is driven by means of carbonic acid gas through an engine and is controlled by an electric wire from shore. The Sims is driven by electricity from a dynamo on shore through a cable to an electric engine in the torpedo. The Brennan is driven and controlled by means of two fine steel wires wound on reels in the torpedo, the reels being geared to the propeller shafts. The wires are led to corresponding reels on shore, and these are rapidly revolved by means of an engine. A brake on each shore reel controls the torpedo. The speed of all these torpedoes is about 19 knots, and their effective range one mile.

A Whitehead was successfully used in the Turco-Russian war of 1877. The Turkish vessel previously mentioned was sunk by one.



Blasting gelatine, dynamite and gun-cotton are capable of many applications to engineering purposes on shore in time of war, and in most cases they are better than powder. They received the serious attention of French engineers during the siege of Paris, and were employed in the various sorties, which were made from the city, in throwing down walls, bursting guns, etc. An explosive for such purposes, and indeed for most military uses, should satisfy the following conditions:

- (1) Very shattering in its effects.
- (2) Insensible to shocks of projectiles.
- (3) Plastic.
- (4) Easy and safe to manipulate.
- (5) Easy to insert a fuze.

(6) Great stability at all natural temperatures and when used in wet localities.

Neither blasting gelatine, dynamite nor gun-cotton fulfil all these conditions; but they satisfy many of them and are more powerful than other substances. For the destruction of walls, trees, rails, bridges, etc., it is simply necessary to attach to them small bags of explosive, which are ignited by means of blasters' fuze and a cap of fulminate of mercury, or by an electric fuze.

We now come to the application of high explosives to warfare in the shape of bursting charges for shells. This is the latest phase of the problem and it is undoubtedly fraught with the most important consequences to both attack and defence. Difficult as it has been to obtain an exact estimate of the force of different explosives under water, the problem is far greater out of the water and under the ordinary conditions of shell fire; the principal obstacle being in the fact that it is physically impossible to control the force of large quantities in order to measure it, and small quantities give irregular results. Theoretically, the matter has been accomplished by Berthelot, the head of the French Government "Commission of Explosives," by calculating the volume of gas produced, heat developed, etc.; and this method is excellent for obtaining a fair idea of the specific pressure of any new explosive that may be brought

forward, and determining whether it is worth while to investigate it further; but the explosives differ so much from each other in point of sensitiveness, weight, physical condition, velocity of explosive wave, influence of temperature and humidity, that we cannot determine from mere theoretical considerations, all that we would like to know. Various methods of arriving at comparative values have been tried, but the figures are very variable, as will be seen by the following tables. Berthelot's commission, some ten years ago, exploded ten to thirty grams of each in 300 pound blocks of lead and measured the increased size of the hole thus made. The relative result was:

No. 1 dynamite, . . . . .	1'
Dry gun-cotton, . . . . .	1'17
Nitro-glycerine, . . . . .	1'20

Powder blew out and could not be measured.

Mr. R. C. Williams, at the Boston Institute of Technology, in the winter of 1888 and 1889, tried the same method, but used six grams in forty-five pound blocks of lead. He obtained a relative result of—

No. 1 dynamite, . . . . .	1'
Dry gun-cotton, . . . . .	1'37
Nitro-glycerine, . . . . .	2'51
Explosive gelatine, . . . . .	2'57
Forcite gelatine, . . . . .	2'7
Warm nitro-glycerine, . . . . .	2'7
Gunpowder, . . . . .	1

The powder gave great trouble in this case, also, by blowing out.

M. Chalon, a French engineer, obtained some years ago, with a small mortar, firing a projectile of thirty kilos and using a charge of ten grams of each explosive, the following ranges:

	<i>Metres.</i>
Blasting-powder, . . . . .	2'6
No. 1 dynamite, . . . . .	31'4
Forcite of 75 per cent. N. G., . . . . .	43'6
Blasting gelatine, . . . . .	45

Roux and Sarrau obtained by experiments in bursting

small bomb shells the following comparative strengths of ranges:

Powder, . . . . .	1'
Gun-cotton, . . . . .	6'5
Nitro-glycerine, . . . . .	10'0

In actual blasting work the results vary altogether with the nature of the material encountered, and with the result that is desired to be accomplished, viz: throwing out, shattering, or mere displacement.

Chalon gives for quarrying :

Powder, . . . . .	1
Dynamite No. 2, containing 50 per cent. nitro-glycerine, . . .	3

For open blasting :

Dynamite No. 3, containing 30 per cent. N. G., . . . . .	1'0
Dynamite No. 1, containing 75 per cent. N. G., . . . . .	2'5
Blasting gelatine, . . . . .	3'5

For tunnelling :

Dynamite No. 3, containing 30 per cent. N. G., . . . . .	1
Dynamite No. 1, containing 75 per cent. N. G., . . . . .	3
Explosive gelatine, . . . . .	19

Finally Berthelot's theoretical calculations give a specific pressure of—

Powder, . . . . .	1
Dynamite, . . . . .	13
Gun-cotton, . . . . .	14
Nitro-glycerine, . . . . .	16
Blasting gelatine, . . . . .	17

It will be observed that the practical results vary largely from the theoretical values, but they seem to indicate that gun-cotton and No. 1 dynamite are very nearly equal to each other, and that in the nitro-glycerine compounds, except where gun-cotton is added, the force appears to be nearly in proportion to the nitro-glycerine contained. From the foregoing it seems fair to estimate roughly the values of bursting charges of shells as follows :

Powder, . . . . .	1
Gun-cotton and dynamite, . . . . .	6 to 10
Nitro-glycerine, . . . . .	13 to 15
Blasting gelatine, . . . . .	15 to 17

Attention has been turned in Europe for more than thirty years towards firing high explosives in shells; but it is only within very late years that results have been reached which are claimed as satisfactory, and it is exceedingly difficult to obtain reliable accounts even of these. Dynamite was fired in Sweden in 1867 in small quantities, and a few years later it was fired in France. But two difficulties soon presented themselves. If the quantity of nitro-glycerine in the dynamite was small, it could be fired in ordinary shells, but the effect was no better than with gunpowder. If the dynamite was stronger in nitro-glycerine, it took but a small quantity to burst the gun. As early as 1864, dry gun-cotton was safely fired in shells in small quantities, but when a sufficient quantity to fill the shell cavity was used, the gun burst. Some few years ago it was found that if the gun-cotton was either wet or soaked in paraffin, it could be fired with safety from powder guns in ordinary shells, provided the quantity was small in proportion to the total weight of the shell, (say) five to six per cent.; but a new difficulty arises from the fact that it breaks the shell up into very small pieces, and it is an unsettled question among artillerists whether more damage is done to an enemy by breaking a shell into comparatively large pieces and dispersing them a long distance with a bursting charge of powder, which has a propulsive force, or by breaking it with a detonating compound into fine pieces, which are not driven nearly so far. When used against troops there is also the objection to the high explosive shell, that it makes scarcely any smoke in bursting, and smoke at this point is useful to the artillerist in rectifying his aim. In the matter of shells for piercing armor, however, there are no two opinions regarding the nature of the bursting charge. To pierce modern armor at all a shell must be made of forged steel, so thick that the capacity of the cavity for the bursting charge is reduced to one-fourth or one-fifth of what it is in the common shell; the result is that a charge of powder is frequently not powerful enough to burst the shell at all; it simply blows the plug out of the filling hole in the rear. In addition it is found that in passing through armor, the heat generated is

so great that the powder is prematurely ignited. If then we can fill the small cavity in the shell with an explosive which will not ignite prematurely, and yet will burst the shell properly after it has passed through the armor, the problem will be solved. Wet or paraffined gun-cotton can be made sluggish enough to satisfy the first condition; but at present the difficulty is to make it explode at all. The more sluggish the gun-cotton, the more powerful must be the fuze exploders to detonate it, and such exploders are themselves liable to premature ignition in passing through the armor. The Italians and Germans claim to have accomplished the desired result up to a thickness of five inches of armor; gun-cotton and fuze both working well. But the English authorities say that no one has yet accomplished it. The Austrians claim to have succeeded in this direction within the last year with a new explosive called ecrastite (supposed to be blasting gelatine combined with sulphate or hydrochlorate of ammonia, and claimed to be one and one-half times as powerful as dynamite). With a gun of 8.24-inch calibre and an armor-piercing shell, weighing 206.6 pounds, containing a bursting charge of 15.88 pounds of ecrastite, they are said to have perforated two plates four inches thick, and entered a third four-inch plate where the shell exploded. There is a weak point in this account in the fact that the powder capacity of the shell is said to be 4.4 pounds. This amount is approximately correct, judging from our own eight-inch armor-piercing shell, but if this is true there could not have been more than nine pounds of ecrastite in the shell instead of sixteen, or else there is an exceedingly small proportion of blasting gelatine in ecrastite, and if that is the case it is not one and one-half times as powerful as dynamite. If it is weak stuff it is probably insensitive, and even if it were strong, one swallow does not make a summer. The English fired quantities of blasting gelatine from a two-inch Nordenfeldt gun in 1884, but when they tried it in a seven-inch gun in 1885, they burst the gun at once. I have only analyzed this Austrian case, because the statement is taken from this year's annual report of the Office of Naval Intelligence, which is an excellent authority, and to illustrate the fact that of the thousands of



accounts, which we see in foreign and domestic newspapers, concerning the successful use of high explosives in shells, fully ninety per cent. are totally unreliable. In many cases they are in the nature of a prospectus from the inventors of explosives or methods of firing, who are aware of the fact that it is almost impossible to dispute any statements that they may choose to make regarding the power of their new compounds, and thinking, as most of them do, that power alone is required.

Referring to the qualities that I have previously cited as being required in a high explosive for military purposes, it is sooner or later found that nearly all the novelties proposed lack some of the essentials and soon disappear from the advertising world only to be succeeded by others. The most common defect is lack of keeping qualities. They will either absorb moisture or will evaporate; or further chemical action will go on among the constituents, making them dangerously sensitive or completely inert, or they will separate mechanically according to their specific gravities.

For further clearness on the subject of the shell charges, which have so far been discussed, the following table is added of weight and sizes of shells for U. S. naval guns, with their bursting charges of powder:

6-inch com. cast-steel shell	3½ to 4 cal. long, wt. 100 lbs., charge	6 lbs.
8 " " " "	" " " " 250 " "	14½ "
10 " " " "	" " " " 500 " "	27 "
12 " " " "	" " " " 850 " "	45 "

#### ARMOR-PIERCING FORGED STEEL SHELL.

6-inch, 3 calibres long, weight 100 lbs., charge	1½ lbs.
8 " " " " 250 " "	3 "
10 " " " " 500 " "	5½ "
12 " " " " 850 " "	11 "

The chief efficiency of small quantities of high explosives having reduced itself to the case of armor-piercing projectiles, it next became evident that there was an entirely new field for high explosives into which powder had entered but little, and this was the introduction of huge torpedo shells which did not rely for their efficiency upon the dispersion

of the pieces of the shell, but upon the devastating force of the bursting charge itself upon everything within the radius of its explosive effect. It is in this field that we may look for the most remarkable results, and it is here that the absolute power of the explosive thrown is of the utmost importance, provided that it can be safely used. Attention was at once turned in Europe to the manufacture of large projectiles with great capacity for bursting charges and it has resulted in the production of a class of shells  $4\frac{1}{2}$  to 6 calibres long, with walls only  $\frac{1}{4}$  of an inch thick. (If they are made thinner they will swell and jam in the gun when fired.)

These shells are used in long guns up to 6 and  $8\frac{1}{2}$  inches calibre and in mortars up to 11.2 inch. They are made from discs of steel, 3 to 4 feet in diameter and 1 inch thick, and are forced into shape by hydraulic presses. The base is usually screwed in, but some of the German shell are made in two halves which screw together. The Italians were the first in this new field of investigation, but the Germans soon followed and after trying various materials were at length reasonably successful with gun-cotton soaked in paraffin. Their 8.4 inch mortar shells of 5 calibres contain 42 pounds; those of 6 calibres contain 57 pounds; and the 11.2 inch mortar shells of 5 calibres contains 110 pounds.

The projectile velocity used with the mortars is about 800 f. s. The effect of these shells against ordinary masonry and earth fortifications is very great. The charge of forty-two pounds has broken through a masonry vault of three feet, four inches thick, covered with two feet, eight inches of cement and with three to five feet of earth over all. The shell containing fifty-seven pounds, at a range of two and one-half miles, broke through a similar vault covered with ten feet of earth; but with seventeen feet of earth the vault resisted. In 1883, experiments at Kummersdorf showed that a shell containing the fifty-seven pound charge would excavate in sand a crater sixteen feet in diameter and eight feet deep with a capacity of twenty-two cubic yards. The Italians have had similar experiences; but it is notable that

in both Germany and Italy several guns and mortars have burst. The velocity in the guns is not believed to exceed 1,200 to 1,300 f. s., and it is not thought that the quantity of gun-cotton is as great in the gun shells as in the mortars. I have lately been informed on good authority that the use of gun-cotton shells has been abandoned in the German navy as too dangerous.

The French, in their investigations in this field, found gun-cotton too inconvenient, and decided upon melenite. This substance has probably attracted more attention in the military world than all others combined, on account of the fabulous qualities that have been ascribed to it. Its composition was for a long time entirely a secret; but it is now thought to consist principally of picric acid, which is formed by the action of nitric acid upon phenol or phenyllic alcohol, a constituent of coal tar. The actual nature of melenite is not positively known, as the French Government, after buying it from the inventor, Turpin, are said to have added other articles and improved it. This is probable, since French experiments in firing against a partially armored vessel, the *Bellequense*, developed an enormous destructive effect, while the English, who afterwards bought it, conducted similar experiments against the *Resistance* and obtained no better results than with powder. The proof that the *Bellequense* experiments were deemed of great value by the French, lies in the fact that they immediately laid down a frigate—*Dupuy de Lome*—in which four-inch armor is used, not only on the side, but about the gun stations, to protect the men; this thickness having been found sufficient to keep out melenite shell. In most armor-clads, the armor is very heavy about the vitals, but the guns are frequently much exposed.

The best authenticated composition for melenite consists of picric acid, gun-cotton and gum-arabic, and lately it is stated that the French have added cresilite to it. Cresilite is another product of coal tar. Melenite is normally only three times as strong as gunpowder; but it is said to owe its destructive qualities in shells to the powerful character of the exploder which ignites it. It has been

known for some years that all explosives (including gun-powder), are capable of two orders of explosion according as they are merely ignited or excited by a weak fuze or as they are powerfully shocked by a more vigorous excitant. Fulminate of mercury has been found most serviceable for the latter purpose. With melenite the French have reproduced all the results that the Germans have effected with gun-cotton and have found that a shell containing 119 pounds of it will penetrate nearly ten feet of solid cement, but will not penetrate armored turrets six to eight inches thick. The French claim that melenite has an advantage over gun-cotton in not being so dangerous to handle and being insensible to shock or friction, and they have obtained a velocity of 1,300 f. s. with the 8.8 inch mortar and claim to have obtained 2,000 f. s. in long guns up to 6.2 inch calibre. However this may be, they are known to have had severe accidents at the manufactory at Belfort and at least one 5.6 inch gun was burst at the *Bellequense* experiments in firing a sixty-six pound shell containing twenty-eight pounds of melenite. The French are said to have large quantities of melenite shells in store, but they are not issued to service.

Probably one reason why we have so many conflicting yet positive accounts of great successes in Europe with torpedo shells, is because each nation wishes its neighbors to think that it is prepared for all eventualities, and they are obliged to keep on hand large quantities of some explosive, whether they have confidence in it or not. Fortunately we are not so situated; but singularly enough what we have done in the field of high explosive projection has been accomplished by private enterprise, and we have attacked the problem at exactly the opposite point from which European nations have undertaken it. While they have assumed that the powder gun with its powerful and relatively irregular pressures was a necessity and have endeavored to modify the explosive to suit it, we have taken the explosive as we have found it, and have adapted the gun to the explosive. At present the prominent weapon in this new field is the pneumatic gun, but it is obvious

that steam, carbonic acid gas, ammonia or any other moderate and regulatable pressure can be used as well as compressed air; it is merely a question of mechanical convenience. In throwing small quantities of certain high explosives, powder guns can be used satisfactorily, but when large quantities are required, the mechanical system of guns possess numerous advantages. All the high explosives are subject to premature detonation by shock; each of them is supposed to have its own peculiar shock to which it is sensitive; but what this shock may be is at present unknown. We do know, however, that premature explosions in guns are more liable to occur when the charge in the shell is large than when it is small; this is due to the fact that when the gun is fired, the inertia of the charge in the shell is overcome by a pressure proportional to the mass and acceleration, which pressure is communicated to the shell charge by the rear surface of the cavity and the pressure per unit of mass will vary inversely as this surface. If then the quantity of explosive in the shell forms a large proportion of the total weight of the shell, we approach in powder guns a condition of shock to it which is always dangerous and frequently fatal. The pressure behind the projectile varies from twelve to fifteen tons per square inch, but it is liable to rise to seventeen and eighteen tons, and in the present state of the manufacture of gunpowder we cannot in ordinary guns regulate it nearer than that. It is not a matter of so much importance so far as the guns are concerned, when using ordinary projectiles, as the gun will endure a pressure of from twenty-five to thirty tons per square inch; but with high explosives in the shell it is a vitally serious matter. From all I can learn regarding European practice, it appears that not only are the explosives made sluggish, but the quantity seldom exceeds thirty per cent. of the weight of the shell and the velocities, notwithstanding, are kept very low. In the pneumatic gun the velocity is low also, but so is the pressure in the gun. The pressure in the firing reservoir is kept at the relatively low figure of 1,000 pounds per square inch or less, and the air is admitted to the chamber of the gun by a balance valve



which cuts off just the quantity of air (within a very few pounds) that is required to make the shot. The gun is long and advantage is taken of the expansion of the air. In no case can the pressure rise in the gun beyond that in the reservoir.

Up to the present time there have been no accidents in using the most powerful explosives in their natural state, and in quantities over fifty per cent. of the weight of the projectile. I have seen projectiles weighing 950 pounds, and containing 500 pounds of explosives (300 pounds of the blasting gelatine and 200 pounds of No. 1 dynamite) thrown nearly a mile and exploded after disappearing under water. According to Gen. Abbot's formula such a projectile would have sunk any armor-clad floating within forty-seven feet of where it struck. Apparently there is no limit to the percentage of explosive that can be placed in the shell except the mechanical one of having the walls thick enough to prevent being crushed by the shock of discharge. In the large projectiles a transverse diaphragm is introduced to strengthen the walls and to subdivide the charge.

The development of the pneumatic gun has been attended with some other important discoveries which may be of interest. It is well known that mortar fire is very inaccurate, except at fixed long distances, in consequence of the high angle, the slowness of flight of the projectile, the variability of the powder pressure, and the inability to change the elevation and the charge of powder rapidly. In the pneumatic gun, the complete control of the pressure remedies the most important of the mortar's defects and makes the fire accurate from long ranges down to within a few yards of the gun. It is obvious that the pressure can be usefully controlled in two ways: (1) by keeping the elevation of the gun fixed and using a valve that can be set to cut off any quantity of air according to the range desired; (2) by keeping the pressure in the reservoir constant, and using a valve which will cut off the same quantity of air every time, changing the elevation of the gun according to the distance. Another important discovery consists in the application of subcalibered projectiles for obtaining increased

range. The gun is smooth bored and a full-sized projectile is a cylinder with hemispherical ends, to the rear of which is attached a shaft having metal vanes placed at an angle, which cause the projectile to revolve round its longer axis during flight. A subcalibered projectile, however, being of less diameter than the bore of the gun, has the vanes on its exterior, and is held in the axis of the gun by means of gas checks which drop off as the projectile leaves the muzzle. The shock to the explosive is, of course, greater than in the full-sized projectile, but the increase can be calculated, and so far a dangerous limit has not been reached. From the fifteen-inch gun, with a pressure of 1,000 pounds per square inch, and a velocity of about 800 f. s., a range of 4,000 yards has been obtained at an elevation of  $30^{\circ} 20'$ , with a ten-inch subcalibered projectile, about eight calibres long and weighing 500 pounds. This projectile will contain 220 pounds of blasting gelatine. With improved full-sized projectiles weighing 1,000 pounds, a range of 2,500 yards will doubtless be obtained. At elevations below  $15^{\circ}$  these long projectiles are liable to ricochet, and what is now wanted is a projectile which will stay under water at all angles of fall and will run parallel to the surface like a locomotive torpedo. Such a projectile has yet to be invented; but I have seen a linked shell which has been experimented with from a nine-inch powder gun, that partially meets this condition. It is made of several sections united by means of rope or electric wire in lengths of 100 or 150 feet. When fired, all sections remain together for some distance; the rear section then first begins to separate; then the next, and so on. It is primarily intended to envelope an enemy's vessel, and to remedy the present uncertainty of elevation in a gun mounted in a pitching boat; but it is found that when it strikes the water in its lengthened out condition, it will neither dive nor ricochet, but will continue for some distance just under the surface until all momentum is lost when it will sink. This projectile is at present crude, and has never been tried loaded, but it will probably be developed into something useful in time.

I have confined my remarks in the foregoing discussion principally to such methods of using high explosives in shells as have proved themselves successful beyond an experimental degree, and practically they reduce themselves to two, viz: using a sluggish explosive in small quantities from an ordinary powder gun, and using any explosive from a pneumatic or other mechanical gun. Naturally, the success of the latter method will soon induce the manufacture of powders having an abnormally low maximum pressure. There is undoubtedly a field for the use of such powders in connection with an air space in the gun to still further regulate the pressure; but nothing of this sort has yet been attempted. Many methods of padding the shell have been devised for reducing the shock in powder guns, but the variability of the powder pressure is too great to have yet rendered any such method successful. A method was patented by Gruson in Germany of filling a shell with the two harmless constituents of an explosive and having them unite and explode by means of a fulminate fuze on striking an object. He used for the constituents nitric acid and dinitro-benzine, and was quite successful; but the system has not met with favor on account of the inconvenience. The explosive was about four times as powerful as gunpowder.

That the advantage of using the most powerful explosives is a real one can be easily shown. The eight-inch pneumatic gun in New York harbor, with a projectile containing fifty pounds of blasting gelatine and five pounds of dynamite, easily sunk a schooner at 1,864 yards range from the torpedo effect of the shell falling alongside of it. This same shell, if filled with gunpowder, would have contained but twenty-five pounds, and have had but one-ninth the power.

The principal European nations are now building armored turrets sunk in enormous masses of cement, as a result of their experiences with gun-cotton and melenite. The fifteen-inch pneumatic projectile, which I described as being capable of sinking an armor-clad at forty-seven feet

from where it struck, would have been capable of penetrating fifty feet of cement had it struck upon a fortification. It was not only a much larger quantity of high explosive than Europeans have experimented with, but the explosive itself is probably more than twice as strong as their gun-cotton and five or six times as strong as their melenite. In the plans of Gen. Brialmont, one of the most eminent of European engineers, he allows in his fortifications about ten feet of cement over casements, magazines, etc. It is evident that this is insufficient for dynamite shells such as I have described.

At Fort Wagner, a sand work built during our war, Gen. Gilmore estimated that he threw one pound of metal for every 3.27 pounds of sand removed. He fired over 122,230 pounds of metal, and one night's work would have repaired the damage. The new fifteen-inch pneumatic shell will contain 600 pounds of blasting gelatine, and judging from the German experiments at Kummsdorf, which I have cited, one of these fifteen-inch shells would throw out a prodigious quantity of sand; either 500 pounds to one of shell, or 2,000 pounds to one of shell, according as the estimate of Gen. Abbot or of Capt. Zalinski is used. The former considers that the radius of destructive effect increases as the square root of the charge; the latter that the area of destructive effect for this kind of work is directly proportional to the charge.

The effect of the high explosives upon horizontal armor is very great; but we have yet to learn how to make it shatter vertical armor. No fact about high explosives is more curious than this, and there is no theory to account for it satisfactorily. As previously stated, the French have found that four inches of vertical armor is ample to keep out the largest melenite shells, and experiments at Annapolis, in 1884, showed that masses of dynamite No. 1, weighing from seventy-five to 100 pounds, could be detonated with impunity when hung against a vertical target composed of a dozen one-inch iron plates bolted together.

In conclusion, I may say that in this country we are prone to think that the perfection of the methods of throwing high

explosives in shell is vastly in favor of an unprotected nation like ourselves, because we could easily make it very uncomfortable for any vessels that might attempt to bombard our sea-coast cities.

This is true as far as it goes, but unfortunately the use of high explosives will not stop there. I lately had explained to me the details of a system which is certainly not impossible for damaging New York from the sea by means of dynamite balloons. The inventor simply proposed to take advantage of the sea-breeze which blows toward New York every summer's afternoon and evening. Without ever coming in sight of land, he could locate his vessel in such a position that his balloons would float directly over the city and let fall a ton or two of dynamite by means of a clock-work attachment. The inventor had all the minor details very plausibly worked out, such as locating by means of pilot balloons the air currents at the proper height for the large balloons, automatic arrangements for keeping the balloon at the proper height after it was let go from the vessel, and so on. His scheme is nothing but the idea of the drifting or current torpedo, which was so popular during our war, transferred to the upper air. An automatic flying machine would be one step farther than this inventor's idea, and would be an exact parallel in the air to the much dreaded locomotive water torpedo of to-day. There seems to be no limit to the possibilities of high explosives when intelligently applied to the warfare of the future, and the advantage will always be on the side of the nation that is best prepared to use them.



PROCEEDINGS  
OF THE  
CHEMICAL SECTION,  
OF THE  
FRANKLIN INSTITUTE.

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[*Stated meeting, held at the institute, Tuesday, January 20, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, January 20, 1891.

Dr. Wm. H. Wahl, president, in the chair.

Members present: Prof. E. F. Smith, Prof. L. B. Hall, Mr. George L. Norris, Dr. Geo. A. Koenig, Dr. E. H. Keiser, Dr. D. K. Tuttle, Dr. S. C. Hooker, Mr. Lee K. Frankel, Mr. H. Pemberton, Jr., Mr. Henry Bower, Mr. W. H. Bower, Mr. L. E. Williams, Prof. Henry Trimble, Mr. A. A. Moore.

The annual report of the treasurer was read and approved.

The resignation of Mr. C. V. Petraeus was received and accepted.

The report of Mr. H. Pemberton, Jr., as chairman of the committee of conference of the Chemical Section, appointed to confer with a similar committee of the American Chemical Society, was read and accepted. This committee was appointed by the president a few days after the last meeting of the section in December, and in response to a request for such a committee from Mr. A. A. Breneman, chairman of the committee of arrangements of the American Chemical Society.

The object of this conference was to consider, in a preliminary way, the question of forming a general organization of American chemists, who should thus constitute a national society. A general meeting of eighteen delegates representing seven different chemical organizations was held in connection with the general meeting of the American Chemical Society at the University of Pennsylvania, on the 30th of December. On the following day, a meeting of the chairmen of the various committees of conference was held at the Manufacturers' Club, in Philadelphia, and, as a result of the two meetings, a set of resolutions was adopted, a copy of which was submitted by Mr. Pemberton as a part of his report to the section.\*

Inasmuch as another meeting of the various conference committees is provided for in the above-mentioned resolutions, the committee was continued for the performance of such duties as were implied in the resolutions. After Mr. Pemberton's report had been accepted, the president extended the thanks of the section to the committee for its action.

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\* Copy of these resolutions is appended.

The following gentlemen were proposed as candidates for membership in the section:

Mr. Geo. Guest Williams, Twenty-fourth and Bainbridge Streets, Philadelphia.

Mr. H. Warren Shepard, 523 Linden Street, Camden, N. J.

Mr. Everett W. Frazer, 915 Ridge Avenue, Philadelphia.

Mr. Edward Gudeman, 931 N. Twelfth Street, Philadelphia.

Mr. Harris Comer, 624 South Washington Square, Philadelphia.

Prof. Albion D. Gray, Penn Charter School, No. 8 S. Twelfth Street, Philadelphia.

Mr. Waldron Shapleigh, Welsbach Incandescent Gas Light Company, Gloucester, N. J.

Dr. Bruno Terné, Baugh & Sons' Chemical Works, Philadelphia.

Mr. Frank H. Rosengarten, Seventeenth and Bainbridge Streets, Philadelphia.

The names were voted upon by the committee on admission, and all were elected.

Mr. Lee K. Frankel then read a paper "On the electrolysis of the metallic sulpho-cyanides." (Referred for publication.)

Prof. Geo. A. Koenig followed with a paper, entitled "Is sulphuric hydrate volatile at ordinary temperatures?" It was also referred for publication.

Prof. Koenig's observations and experiments tended towards an affirmative answer of the question which formed the title of his paper, and some interesting discussion was evoked.

Dr. Tuttle referred to recorded experiments, in which the absence of change in solutions of barium salts, exposed to the possible evaporation of sulphuric acid tended to prove the non-volatility of the acid. Prof. Smith cited some observations made by himself in connection with electrically deposited mercury, in which a weight of 0.3 grams of that metal lost in the course of something less than twenty-four hours a weight of 0.002 grams presumably by evaporation.

The paper by Dr. Hans v. Strombeck, "On the specific heat of brines of different specific gravity; and on the determination of the specific heat of a body generally," was read by title and referred for publication.

A paper from Mr. J. M. Emanuel, U. S. N., "On the occurrence of amberite, ambrite or fossil gum, in a coal seam at Hawakawa colliery, Bay of Islands, N. Z., was read by the secretary, and was referred for publication.

Prof. Smith then presented a paper "On the electrolytic separation of certain metals in phosphate solutions." It was referred for publication.

After a number of interesting comments upon Prof. Smith's contribution, the section adjourned.

WM. C. DAY, *Secretary*.

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*Resolutions adopted at a conference of chemists, assembled for considering the formation of a national organization, held December 30 and 31, 1890, at the University of Pennsylvania, Philadelphia.*

About eighteen delegates from the following chemical societies were present; to wit:

(1) American Association for the Advancement of Science, Chemical Section.

(2) American Chemical Society.

(3) Franklin Institute, Chemical Section.

(4) Brooklyn Institute, Chemical Section.

(5) Association of Official Agricultural Chemists.

(6) Chemical Society of Washington.

(7) Manufacturing Chemists' Association of the United States.

Prof. A. B. Prescott in the chair.

*Resolution 1.* It is desirable that an American Association of chemists be formed to embrace all existing American chemical organizations.

*Resolution 2.* Resolved that this conference recommend to all existing American chemical organizations that they call a meeting of their bodies to be held in Washington in connection with the meeting of the American Association for the Advancement of Science for 1891, and that each of these organizations be requested to appoint a committee, or to continue their present committee for the further discussion of the subject submitted to the conference now in session.

*Resolution 3.* Resolved that this general conference committee composed of the present sub-committees or such others as may be appointed by the several organizations, be called together at as early a time as practicable before the joint meeting recommended in the resolution adopted.

*Resolution 4.* Resolved that meanwhile each sub-committee, through its chairman, shall formulate such modifications of the constitution of the American Chemical Society as it shall deem necessary to adapt it to the requirements of the association proposed.

*Resolution 5.* Resolved that the chairmen of these sub-committees shall then, so far as possible, harmonize the views embodied in these reports of their several organizations, and shall have printed for presentation at the joint meeting a report, or majority and minority reports, on a constitution for the proposed association of American chemists.

Voted that the chairman of this conference, with Prof. Clarke and Prof. Hale, be a committee of three to select time and place for the meeting called for in resolution No. 3.

Voted to request the American Chemical Society to print the minutes of this conference in their proceedings of the Philadelphia meeting, and to mail copies as widely as possible to chemists in North America.

The secretary of this conference was desired to communicate the above resolutions to scientific journals with a view to obtain a wide publication of the same.

Adjourned to meet at call of the chair.

H. CARRINGTON BOLTON,

Secretary.

## COMMITTEE OF CONFERENCE, WITH SOCIETIES REPRESENTED.

Dr. A. C. Peale, United States Geological Survey, Washington, D. C., representing the Chemical Society of Washington.

Prof. F. W. Clarke, United States Geological Survey, Washington, D. C., representing the American Association for the Advancement of Science.

Dr. H. W. Wiley, Department of Agriculture, Washington, D. C., representing the Official Agricultural Chemists' Association.

Mr. H. Pemberton, Jr., Twenty-ninth and Gray's Ferry Road, Philadelphia, Pa., representing the Chemical Section of the Franklin Institute.

Mr. Henry Bower, Twenty-ninth and Gray's Ferry Road, Philadelphia, Pa., representing the Manufacturing Chemists' Association of the United States.

Dr. Walter H. Kent, 64 Livingston Street, Brooklyn, N. Y., representing the Brooklyn Institute.

Dr. E. Waller, School of Mines, Columbia College, New York city, representing the American Chemical Society.

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## THE ELECTROLYSIS OF THE METALLIC SULPHO-CYANIDES.

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BY LEE K. FRANKEL.

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[Read before the Chemical Section, January 20, 1891.]

In a paper published by Dr. Smith and the author, in the Franklin Institute *Journal* for August, 1889, the statement is made that manganese, when deposited from a solution containing an excess of potassium sulphocyanide, separates as a grayish white compact deposit on the negative pole, differing in this respect from solutions of the nitrate or the sulphate from which manganese is deposited as oxide. Furthermore, it is stated that nickel, cobalt, iron and several other metals separate very rapidly from cold sulphocyanide solutions under the influence of a weak current. In the following paper, the above experiments have been repeated, and the action of the current has been tried on various other metallic sulphocyanides. The results obtained have been, for the most part, of a negative order, but are here appended, as they illustrate some new facts in electro-chemical analysis.

*Decomposition of Alkaline Sulphocyanides.*—If a concentrated aqueous solution of potassium or ammonium sulpho-

cyanides (300 grams in one litre) is subjected to the action of a weak current (1.5 cc. OH gas per minute), the solution at first remains clear. After from one to two hours it gradually becomes turbid, and an odor of hydrocyanic acid is readily perceptible. If the current is allowed to run for twelve hours, the turbidity constantly increases until, at length, the solution is filled with a lemon-yellow amorphous deposit. According to Bunge (*Ber.* 3, 297), who investigated the action of the current on potassium sulphocyanide, and to Lidow (*Ber.* 17, ref. 252), who obtained similar results with ammonium sulphocyanide, this yellow deposit is pseudosulphocyanogen.

If, however, the solution, while being electrolyzed, is heated on a water-bath, there is evolved along with the pseudosulphocyanogen an unbearable sickening odor, due in all probability to the formation of organic sulphur compounds. In dilute sulphocyanide solutions, however, the formation of the lemon-yellow compound takes place in but small quantities, and only after the lapse of considerable time, so that if the current be sufficiently weak and the solution sufficiently dilute, no turbidity whatever occurs in the solution. It was anticipated that this behavior of the dilute solution could be utilized for the electrolytic deposition of the metals, but the other products of decomposition exerted such an influence on the solution that in but very few cases were quantitative results obtained. These are given below as they were found.

*Mercury.*—Mercury can be completely deposited from solutions of potassium or ammonium sulphocyanide. The metal separates very rapidly from the respective solutions, and resembles the deposit obtained in the electrolysis of the double cyanide of mercury and potassium, in that it never possesses a drop-like appearance, but is dense and coherent.

In the first determination a slight turbidity occurred, due to the larger quantity of ammonium sulphocyanide. The filtrates from all the four determinations gave the blood-red color with ferric chloride, but did not darken on the addition of ammonium sulphide. The deposits were washed.



first with cold then with hot water, and eventually dried on the edge of a warm iron plate.

	Mercury Present in Grams.	Ammonium Sulphocyanide in Grams.	Time.	Current per Minute.	Dilution.	Mercury Found in Grams.
(1)	'2292	1'5	16 hours.	1' cc. OH gas	75 cc.	'2289
(2)	'2292	'9	16 hours.	1'4 cc. OH gas	75 cc.	'2285
(3)	'2292	'9	16 hours.	'65 cc. OH gas	75 cc.	'2295
(4)	'2292	1'2	16 hours.	'65 cc. OH gas	75 cc.	'2294

*Gold.*—Like mercury, gold can be completely deposited from a solution of ammonium sulphocyanide. The metal separates as a compact, firmly adherent deposit, which can readily be washed with either hot or cold water. The conditions, under which the deposits were obtained, are as follows:

(1) To a solution of gold chloride containing 0'0185 gram of gold, 1'5 grams of ammonium sulphocyanide were added, along with 150 cc. of water. A current of 1 cc. OH gas per minute was allowed to act for six hours, at the end of which time, the metal was completely deposited. The gold found weighed 0'0183 gram.

(2) The conditions in this determination were like the above, excepting that the gold chloride solution contained '1443 gram of gold, and that the current was allowed to act for sixteen hours. The resultant gold weighed '1439 gram.

*Cadmium.*—If ammonium sulphocyanide is added to a solution of cadmium sulphate, the solution remains clear. If this solution be now subjected to the action of the current, the cadmium immediately deposits as metal, but in a very spongy form. This sponginess is frequently of such an extent, that the deposit spreads in a continuous layer from the negative to the positive pole, and readily detaches itself when washed. Various conditions of current and solution were tried in order to remedy this result, the current used varying from 0'25 cc. to 2'2 cc. OH gas per minute, and the amount of ammonium sulphocyanide from '3 gram to

six grams in solutions diluted from fifty to 200 cc. In all cases, however, the cadmium deposited in a spongy form, and the method had to be abandoned.

*Palladium, Nickel, Cobalt and Zinc.*—In the electrolysis of neutral solutions of these four metals in the presence of ammonium sulphocyanide, similar reactions took place. In each instance, the metal began to deposit immediately with currents ranging from one to two cc. OH gas per minute, in solutions averaging one gram of ammonium sulphocyanide. In the case of the nickel and the cobalt, the metals deposited in a compact metallic form, and frequently could be separated as a thin film from the sides of the platinum dish. With all four metals, however, the deposition continued only to a certain point, after which a reverse action took place and the coatings gradually dissolved. In the case of the zinc, after permitting the current to act for twenty-four hours, every portion of the gray metallic deposit obtained in the earlier stage of the operation had gone into solution. The probable cause for this singular action lies in the fact that in the decomposition of the alkaline sulphocyanides, there is formed an estimable amount of alkaline cyanide which, in the experiments above referred to, could be easily recognized by its odor. The addition of ammonia to the solution from time to time during the passing of the current, gave no beneficial results, and all attempts to deposit the metals completely, with the strength of current used, proved futile.

*Iron and Manganese.*—The continuation of the investigation previously reported on these metals, led to no satisfactory results. As has been stated, the metals can be deposited from solutions of their sulphocyanides, but not completely, and the tendency to oxidize is so great, that films of the oxides form on the edges of the solutions, even during the action of the current. As soon as the solutions are poured off, and the surfaces of the metals are exposed to the air, these films of oxide rapidly spread over the entire deposits, and exclude any possibility of the estimation of iron or manganese by this method.

*Arsenic.*—Varying quantities of ammonium sulphocyanide

were added to solutions of sodium arsenate and of sodium arsenite made alkaline with sodium hydrate. In no case was there the faintest deposition of metal.

*Lead.*—It has already been shown that when a solution of lead nitrate is subjected to the action of the electric current, the lead will deposit as peroxide on the positive pole. If, now, ammonium sulphocyanide be added to the solution, while the current is passing the lead will deport itself similarly to the manganese, in that the peroxide of lead gradually dissolves off the anode, while the metal deposits on the cathode. This deposition of metal will continue until no more lead can be detected in the filtrate, but, while the greater portion of the lead has separated as metal on the negative pole, a white coating will now be found on the positive pole, which after solution in acids, gives a reaction with hydrogen sulphide for lead. This coating on the anode prevents any quantitative estimation of lead in an ammonium sulphocyanide solution. The metal as deposited from such a solution has a gray metallic appearance, is but loosely adherent to the dish, and easily becomes covered with a yellow film of oxide.

*Antimony, Bismuth and Tin.*—To the hydrochloric acid solutions of these metals, ammonia was added until precipitates formed, varying quantities of ammonium sulphocyanide were then added to the respective solutions, followed by hydrochloric acid, until the precipitates just dissolved. All three metals deposit rapidly from such solutions, the tin, however, but incompletely. The filtrate from the bismuth gave no reaction for the metal, but the deposit was so spongy that it could not be handled. The antimony deposit detached itself so readily from the sides of the dish on which it separated, that no effort was made to discover if quantitative results could be obtained.

*Pseudosulphocyanogen.*—In a paper on the preparation of a new yellow dye color, H. O. Miller (*Ber.* 18, ref. 676) states that the impure compounds, which he designates "Kanarin," is obtained by the action of hydrochloric acid and potassium chlorate on potassium sulphocyanide. The resulting yellow substance dissolves in potassium hydrate

solution, from which alcohol deposits the potassium salt. If the latter is filtered off and dissolved in water, hydrochloric acid reprecipitates the dyestuff. As noted above, Lidow (*Ber.* 17, ref. 252) asserts that the yellow amorphous compound obtained in the electrolysis of ammonium sulphocyanide is pseudosulphocyanogen, the yield being greatest at 50° C., and, furthermore, states that kanarin and pseudosulphocyanogen are identical.

In a still later paper, W. Markonikow (*Ber.* 17, ref. 279) denies that kanarin and pseudosulphocyanogen are identical bodies. In proof of this, he states that if pseudosulphocyanogen is dissolved in an equal volume of potassium hydrate diluted with twenty parts of water, it is converted into potassium sulphocyanide and potassium cyanate, while if kanarin undergoes a similar treatment, the potassium salt is obtained, which, as above stated, can be precipitated by alcohol. To prove the correctness or fallacy of these statements, the following experiments were made:

A solution of ammonium sulphocyanide, containing 300 grams of the salt dissolved in a litre of water, was electrolyzed with a current generating two cc. OH gas per minute, for forty-eight hours. The yellow compound, which had separated, was filtered off and washed with hot water, until the washings no longer gave the blood-red coloration with a solution of ferric chloride, and dried at 100° C. The resulting lemon-yellow compound dissolved readily in concentrated sulphuric acid, but without any evolution of sulphur dioxide, a reaction which Markonikow gives as distinctive for kanarin. The yellow substance dissolves in potassium hydrate, more readily on heating, to a yellow solution. A portion of this solution, after acidifying with dilute nitric acid, and the addition of ferric chloride, gave not the faintest reaction for potassium sulphocyanide. To the remainder of the solution, alcohol was added. The turbidity that formed, gradually settled to the bottom of the liquid in oily drops. These were separated from the supernatant liquid by means of a separatory funnel, dissolved in water, and from this solution the lemon-yellow compound thrown out by means of hydrochloric acid. If

these reactions are indicative, kanarin and pseudosulphocyanogen are identical, but the compound obtained in the electrolysis of ammonium sulphocyanide is not pseudosulphocyanogen.

The action of potassium chlorate and hydrochloric acid on ammonium sulphocyanide was also tried, the results being similar to those obtained by Miller and Markonikow with potassium sulphocyanide. The compound thus obtained has a reddish yellow color, dissolves in potassium hydrate, the resulting solution giving a reaction for potassium sulphocyanide, and forming the potassium salt with alcohol, showing that the original substance is impure. The potassium salt dissolved in water, and treated with hydrochloric acid, now gives a lemon-yellow precipitate resembling in color that obtained by the electrolysis of the ammonium sulphocyanide.

The author reserves, for a future communication, the investigation of the composition of these compounds, and in conclusion wishes to extend his thanks to Dr. Edgar F. Smith, under whose supervision, and at whose instance, the above work was undertaken.

UNIVERSITY OF PENNSYLVANIA,  
PHILADELPHIA, January 13, 1891.



ANNUAL REPORT OF THE BOARD OF MANAGERS OF THE  
FRANKLIN INSTITUTE.

(For the year 1890.)

The Board of Managers of the Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts, respectfully presents the following report of the operations of the institute for the year 1890 :

## MEMBERS.

Members at the close of 1889, . . . . .	2,224	
Number of new members elected who have paid their dues, . . . . .	192	
	<u>192</u>	2,416
Lost by death or resignation, . . . . .	89	
Dropped for non-payment of dues, . . . . .	158	
	<u>247</u>	
Total membership at close of 1890, . . . . .		2,169

## FINANCIAL STATEMENT.

Balance on hand January 1, 1890, . . . . . \$2,172 33

*Receipts :*

Contributions from members, . . . . .	\$6,679 75	
Certificates of stock, second class, . . . . .	30 00	
Interest on investments of institute's funds, . . . . .	1,654 00	
Interest on investments of building fund, . . . . .	353 09	
Temporary loan, . . . . .	2,000 00	
Cash from other sources, . . . . .	9,264 39	
	<u>19,981 23</u>	
		\$22,153 56

*Payments :*

Committee on library, . . . . .	\$2,366 13	
Committee on instruction, . . . . .	2,236 77	
Curators, . . . . .	1,938 37	
Salaries and wages, . . . . .	4,145 00	
Insurance, . . . . .	450 00	
Temporary loan, . . . . .	2,000 00	
Interest on temporary loan, . . . . .	60 97	
Other expenditures, . . . . .	8,298 61	
	<u>21,495 85</u>	
Balance on hand December 31, 1890, . . . . .		\$657 71

## LIBRARY.

The annual report of the committee on library shows that the additions to the institute's collections of books, pamphlets, maps, etc., during the past year were fully up to the average of recent years, the total increase amounting to 3,229 numbers.

There has been no change in the policy of the committee in respect of the use of the library by the public. The plan in force now for three years is found to work well and gives general satisfaction. The best evidences of this are the increasing use of the library by non-members, and the fact that no case has thus far occurred of the abuse of the privileges accorded to non-members. For the details of the year's operations the board refers to the committee's report.

## COMMITTEE ON SCIENCE AND THE ARTS.

This committee has continued the excellent work recorded in last year's report. The efficiency of its present methods, and the thoroughness of its investigations are plainly visible in the character of the subjects submitted for examination. A number of the committee's reports appears in the *Journal* and reflects credit upon the institute. The excellent work accomplished by this committee since its reorganization in 1887, has fully justified the expectations of those who anticipated that further usefulness would result from the changes then made.

The members are referred, for details of the committee's work, to the chairman's report.

## SECTIONS.

The Chemical Section continues in the flourishing condition noted in the report of 1889. Its activity is apparent in the pages of the *Journal* and the board expresses its gratification at the excellent record the section has made.

Near the close of the year 1890, the board favorably acted on the petition of a number of members of the institute to establish an electrical section, and the institute at its last stated meeting approved the action. The board may hope, therefore, that in the next annual report the new section will be referred to as another useful auxiliary in furthering the objects of the institute.

The details of the work of the Chemical Section may be found in its annual report.

## THE JOURNAL.

The board refers with satisfaction to the condition of the *Journal*, both in respect of the manner in which the committee on publications and the secretary have sustained its high reputation, and in respect of its business condition. In regard to the last, the board may report in view of the steady improvement that has been going on in the past few years, that the *Journal* is self-supporting, and there is no reason why, with the same intelligent direction of its affairs, it should not so continue. The board again takes the opportunity of commending the *Journal* to the members of the institute as worthy of their generous support.

## LECTURES.

The committee on instruction has adhered generally to the policy of arranging a series of lectures, with especial reference to the presentation of the latest and most important subjects of technical interest. The character of many of these lectures has been such as to afford admirable material for the *Journal*, and to contribute to the reputation of the institute. In this work, the committee, through the able efforts of its professors, has been able, as heretofore, to command the services of many eminent scientists and engineers.

During the past year the following lectures were delivered :

- |          |     |   |
|----------|-----|---|
| January  | 6.  | Prof. F. W. Clarke, "The Chemistry of Coal."  |
|          | 13. | Prof. R. L. Chase, "A Revolution in Dyeing."  |
|          | 17. | Prof. W. Le Conte W. Stevens, "The Mammoth Cave of Kentucky."                                 |
|          | 20. | Lieut. Bradley A. Fiske, "Electricity in Warfare."  |
|          | 27. | Dr. W. Thomson, "Color-Blindness."  |
|          | 31. | Mr. C. John Hexamer, "A Descriptive and Illustrated Sketch of Germany."                       |
| February | 3.  | Dr. Louis Duncan, "Modern Conceptions of Electricity."  |
|          | 7.  | Mr. Theo. C. Search, "Wool, from the Fleece to the Card."                                     |
|          | 10. | Mr. Eugene Griffin, "Electric Railways."  |
|          | 14. | Mr. Theo. C. Search, "Wool, from the Card to the Fabric."                                     |
|          | 17. | Mr. Geo. F. Kunz, "Precious Stones."  |
|          | 21. | Mr. Wm. F. Durfee, "Diamond Drilling."  |
|          | 24. | Dr. Chas. B. Dudley, "Bearing-Metal Alloys."  |
|          | 28. | Mr. Fred. E. Ives, "Street Scenes in Italy, caught with the Camera."                          |
| November | 10. | Prof. Persifor Frazer, "Some Helps and Hinderances to the Progress of Theoretical Chemistry." |
|          | 14. | Mr. C. John Hexamer, "From the Baltic to the Volga."  |
|          | 17. | Dr. H. Hensoldt, "Some Geological and Cosmical Problems."                                     |
|          | 21. | Prof. Edward D. Cope, "The Zoölogical Characters of Man."                                     |
|          | 24. | Dr. Bruno Terne, "Ammonia: its Sources and Technical Uses."                                   |
|          | 28. | Mr. Edwin S. Checkley, "A Natural Method of Physical Culture."                                |
| December | 1.  | Prof. G. G. Groff, "The Organization of Sanitary Work in Cases of National Disaster."         |
|          | 5.  | Dr. L. Webster Fox, "Eyesight: its Care during Infancy and Youth."                            |
|          | 8.  | Capt. F. A. Mahan, "Philadelphia as a Seaport."   |
|          | 12. | Mr. J. C. Trautwine, "Engineering Notes from Europe."   |
|          | 15. | Prof. Wm. D. Marks, "Two and a Half Miles a Minute."  |
|          | 19. | Mr. Fred. E. Ives, "Photography in the Colors of Nature."                                     |
|          | 29. | Dr. A. Victoria Scott (a Christmas lecture for children), "A Trip to Alaska."                 |

## DRAWING SCHOOL.

The number of pupils in the drawing school was the largest in its history, and the board expresses its entire satisfaction with its condition.

The figures representing the attendance of pupils are as follows :

At the spring term of 1890, . . . . .	178
At the winter term, . . . . .	295
Total attendance for the year, . . . . .	473

being an increase of 86 over the number for the year 1889.

## STATE WEATHER SERVICE.

The work of this branch of the institute has been continued as heretofore. The liberal grant of funds by the state for the carrying on of this service has enabled the committee on meteorology to make more elaborate and valuable publications of the results of its work, and steadily to increase the efficiency of the service. The value of the service is generally recognized and there is every reason to believe that it will become a permanent institution. The change made by the last Congress in transferring the United States Weather Service to the Department of Agriculture is not expected to cause any modification in the relations of the Washington Bureau to the several State Weather Services.

## GENERAL REMARKS.

The board regrets that the great increase in receipts from new memberships as shown by last year's report and which there was every reason to hope would continue, does not appear this year. As a consequence, the financial condition of the institute at the end of 1890 is not as gratifying as at its beginning. Practically, there is no more feasible way of immediately benefiting the institute than by obtaining new members, and with the large field from which to draw and the little effort necessary to bring them in, it is a matter of some mortification that better results are not obtained.

The board reports receipt of a legacy from the late Geo. S. Pepper, amounting to about \$23,815.05, and the prospect of an additional sum from the residuary. The income from this legacy will be of assistance in the maintenance of the institute work.

The board deplores the failure of the effort that has been made to obtain subscriptions to the building fund, conditioned upon the sum of \$100,000 being raised before the end of the year 1890. The financial state of the country during the past few months has made success impossible. It is hoped that with renewed prosperity in business circles a second effort may be made and the desired result attained. Nothing stronger can be said upon this subject than that contained in the last report, and the board urges upon the committee in charge to recommence its work with fresh vigor as soon as practicable.

In closing its report, the board desires to express its grateful acknowledgment of the benefits accruing to the library from the increased income

available from the Bloomfield H. Moore fund, which, through the recent gift of Mrs. Clara Bloomfield Moore, has been almost doubled.

A like expression of grateful acknowledgment is due, and is hereby tendered, to the family and executors of the late George S. Pepper.

By order of the board.

JOSEPH M. WILSON, *President*.

# ABSTRACT FROM THE ANNUAL REPORT OF THE COMMITTEE ON LIBRARY.

(For the year 1890.)

During the year the following additions were made to the library :

Volumes bound, . . . . .	1,080	
Volumes unbound, . . . . .	630	
Pamphlets, . . . . .	1,350	
Total, . . . . .		3,060
Maps and charts, . . . . .	179	
Photographs and lithographs, . . . . .	11	
Newspaper clippings, . . . . .	4	
Circulars, . . . . .	45	

Total additions of all kinds, . . . . . 3,299

Total number of volumes in the library at the close of

1889, . . . . . 33,305

Additions, bound and unbound volumes, 1890, . . . . . 1,710

Total number of volumes in the library at close of

1890, . . . . . 35,015

*Pamphlets*.—Total number in the library, December 31,

1889, . . . . . 19,622

Add those received in 1890, . . . . . 1,350

Total, . . . . . 20,972

Total number of maps and charts at the close of 1890, . . . . . 1,962

Designs and drawings, . . . . . 603

Photographs and lithographs, . . . . . 1,050

Newspaper clippings, . . . . . 116

Circulars, . . . . . 98

Manuscripts, . . . . . 21

*Duplicates*.—The number of duplicates on hand at the close of 1890:

Volumes, 2,762; pamphlets, 5,857; charts, 169; periodicals, 8,022.

*Moore fund*.—During the year 132 volumes, 126 unbound volumes and 4 pamphlets were purchased with this fund. Six volumes were rebound and one was repaired.



*Public use of the library.*—891 receipts were filed by non-members during the year, consulting 2,472 volumes of patents, 534 numbers of the official gazette, 942 volumes and 43 pamphlets from the other departments of the library.

*Circulation.*—2,037 volumes were issued to members for home use, an increase of 348 over 1889, and 788 over 1888.

*Exchanges.*—The number during 1890 amounted to 380, an increase of 4 compared with the previous year.

By order of the Committee on Library,

WM. H. WAHL, *Secretary.*

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## ANNUAL REPORT OF THE COMMITTEE ON SCIENCE AND THE ARTS.

The chairman of the Committee on Science and the Arts respectfully makes the following report of the work of the committee for the year 1890 :

The total number of new applications received during the year was sixty-two, of which fifty-five were approved for investigation, six rejected by the preliminary committee and one withdrawn. Thirty-four reports have been acted upon during the year and forty are still pending.

In four cases the award of the Elliott Cresson medal was recommended ; in eleven, the John Scott legacy medal and premium, and in two, certificates of merit, and in the reports completed and now pending there are two awards of the Elliott Cresson medal and three of the John Scott legacy medal and premium.

Protests have been received to three of the awards of the committee, two of which, after investigation, were not sustained, and the third was withdrawn.

The foundation of the Edward Longstreth medal of merit, through the liberality of Mr. Edward Longstreth, will add greatly to the resources of the committee, in enabling it to make an award in many deserving cases which the regulations governing the awards, heretofore in the control of the committee, prohibited.

The committee deemed it best to amend its by-laws by inserting a provision forbidding members of the committee to receive awards except in cases where the institute directed that the investigation should be made.

The committee has undertaken to investigate the subject of hot-water boilers for heating purposes, and a sub-committee is now at work preparing a code for testing such boilers.

The index of the work of the committee from 1834 to 1890 has been completed and published, and the reports are now available for reference. The thanks of the committee are due to Mr. S. Lloyd Wiegand, one of its members, for the interest and labor he has expended in the execution of this work.

Respectfully submitted,

H. W. SPANGLER,

*Ch. Com. Sci. and Arts.*

REPORT OF THE CHEMICAL SECTION OF THE FRANKLIN  
INSTITUTE FOR THE YEAR 1890.

*Mr. Joseph M. Wilson, president Franklin Institute.*

SIR:—I have the honor to submit the following report of the proceedings of the Chemical Section, for the year 1890.

The progress of the section during the year has been highly gratifying to its members, and while the report for 1889 showed for that period a condition of prosperity unprecedented in the history of the section, it is none the less true that the past year has entirely fulfilled the sanguine expectations of its members and all others interested in its progress and welfare.

The work of the section was prosecuted with the aid of the following officers:

<i>President,</i> . . . . .	Mr. T. C. PALMER.
<i>Vice-presidents,</i> . . . . .	{ DR. H. F. KELLER, MR. W. L. ROWLAND.
<i>Secretary,</i> . . . . .	DR. WM. C. DAY.
<i>Treasurer,</i> . . . . .	DR. H. W. JAYNE.
<i>Conservator,</i> . . . . .	DR. WM. H. WAHL.

Stated meetings were held, as required by the by-laws of the section, on the third Tuesday of each month of the year, excepting July and August.

*Membership.*—The number of members, according to the treasurer's books, at the beginning of the year, was seventy-one; the number at present enrolled is seventy-two, three new members having been elected, while two have resigned.

*Finances.*—The report of the treasurer, submitted to the section at its last meeting, shows total receipts from

various sources during the year amounting to . . . . .	\$138 00
Cash on hand at beginning of the year, . . . . .	91 50
	<hr/>
	\$229 50
Disbursements amounted to . . . . .	132 17
	<hr/>
thus leaving a cash balance of . . . . .	\$97 33

in the hands of the treasurer at the end of the year.

*Attendance at meetings.*—The meetings have been well attended by the members, and in almost every case the presence of a number of visitors has served to indicate the interest taken in the section by its outside friends.

*Journals received by the section.*—Early in the year the library committee authorized the purchase for the section of a complete set of *Liebig's Annalen* from 1832 to 1886. Subscription for this journal, beginning with 1887, was made by the section; this periodical, so essential as an auxiliary to scientific investigation, is therefore now complete.

Owing to the very satisfactory condition of the section's treasury, it has been found practicable during the year to add to the list of journals previously taken by the section the following, which are at present regularly received, viz: *Chemisches Centralblatt*, *Liebig's Annalen der Chemie*, *Zeitschrift für physiologische Chemie*, *Zeitschrift für Krystallographie*, *Tschermak's mineral-*

*ogische Mittheilungen, Bulletin de la Société Chimique, Gazzetta Chimica Italiana.*

That the influence of the Chemical Section is making itself felt not only in Philadelphia and neighboring cities, but throughout the country, follows from the expressions of interest which in one way or another frequently have come to the notice of its members. Papers from writers in distant cities have been forwarded for consideration by the section and subsequent publication in the *Journal*.

Applications for membership from persons at a distance have also been made.

In view of these facts, the section has, at a recent meeting, appointed a committee for the purpose of considering means by which the attention of chemists at large may be called to the facilities for prompt publication which the institute possesses.

During the year, sixteen important papers, giving the results of original investigations, have been read before the section. Nine communications of a miscellaneous character have been presented; thus making in all twenty-five matters of scientific interest which have occupied the attention of the section.

Very respectfully submitted,

WM. C. DAY, *Secretary*.

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## Franklin Institute.

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[*Proceedings of the annual meeting, held Wednesday, January 21, 1891.*]

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, January 21, 1891.

JOS. M. WILSON, president, in the chair.

Present, 244 members and twenty-two visitors.

Additions to membership since last report, eleven.

The annual reports of the Board of Managers, the Committee on Library, the Committee on Science and the Arts and the Chemical Section were presented and accepted. (For these reports, see *ante*.)

The secretary, by direction of the Committee on Science and the Arts, presented the report of the committee in reference to the regulations for the grant of the Edward Longstreth medal of merit. On motion the committee's action was confirmed and the regulations proposed in the report were approved.

On motion, the grant of the Edward Longstreth medal of merit, recommended by the committee to the Menlo Park Ceramic Works, for their decorative tiles and faience work, was approved.

A letter from Dr. Francis Fowler, of Washington, D. C., was read, accepting his election as an honorary member.

Mr. S. Lloyd Wiegand read a paper on the Olsen testing machine. The speaker described the machine with the aid of lantern slides, exhibiting the

details of construction, and illustrated its operation by reference to a machine of large capacity that was mounted upon the platform. Several pieces of steel were tested by tensile stress, and an interesting feature of the machine, by which a continuous graphical record of the behavior of the test-piece is made, was exhibited. The speaker alluded to the fact that the Committee on Science and the Arts had lately recommended the grant of the Elliott Cresson medal to the inventor of this machine. (Referred for publication.)

Mr. Wm. B. LeVan presented an abstract of a paper, entitled *Fast Trains*, in which he gave an account of some remarkable performances of English locomotives, and advocated higher speeds upon American railways. (Referred for publication.)

Mr. Edmund Stirling exhibited and described briefly, a suite of colored lantern slides of Japanese subjects, made by native artists. These views were loaned for the purpose by Dr. Edward Williams, and were greatly admired for the exquisite delicacy of the coloring.

Mr. Frederick E. Ives, by request, exhibited, with the lantern, some specimens of photographs in the colors of nature, made by his process of heliochromy, and briefly described his method.

The secretary referred to the receipt of a communication from Mr. J. M. Emanuel, P. A. Eng., U. S. N., member of the institute, describing the occurrence of amberite (or ambrite), a fossil gum, in a specimen of coal from New Zealand. A specimen of the coal containing a mass of the gum was shown.

The secretary exhibited and described a flexible metallic tube, invented by Mr. T. R. Almond, of Brooklyn, N. Y.

The following amendments to the by-laws of the institute, recommended for adoption by the managers, were read, viz :

*Section 8 of Article III*, shall read as follows: Non-resident members shall be those who reside permanently at a distance not less than fifty miles from Philadelphia.

Holders of second class stock, contributing members and candidates for membership, wishing to be classed as non-resident members, are required to present a request therefor to the Board of Managers, which may at its discretion grant the request. No person, however, shall be enrolled as a non-resident member, who may reside for a portion of the year in Philadelphia, and no request for such enrolment shall be granted to any holder of second class stock or contributing member who may temporarily be absent from the city for a period of less than one year.

*Section 1, Article IV*.—Insert after the words five dollars, "except as hereinafter provided." [The section, as amended, would read: "Every member, other than a holder of second class stock, shall pay an annual contribution of five dollars, except as hereinafter provided, but the payment of fifty dollars within any one year shall constitute a member for life, with an exemption from all annual payments.]"

*Section 2, Article IV*, shall read as follows: Holders of second class stock and contributing members enrolled as non-resident members shall pay upon election to membership an entrance fee of five dollars and an annual fee of two dollars.

Present *Section 2, of Article IV*, shall be designated Section 3.

Present *Section 3, of Article IV*, shall be designated Section 4.

The amendments were ordered to be published in accordance with prescribed usage.

The tellers of the annual election made their report of the result of the election held this day, between the hours of 4 and 8 p.m., and the following were thereupon declared elected :

<i>President</i>	(to serve one year), . . . .	Joseph M. Wilson.
<i>Vice-president</i>	( " three years), . . . .	Wm. P. Tatham.
<i>Secretary</i>	( " one year), . . . .	Wm. H. Wahl.
<i>Treasurer</i>	( " " " ), . . . .	Samuel Sartain.
<i>Auditor</i>	( " three years), . . . .	Wm. O. Griggs.

*Managers* (to serve three years) :

Henry Bower,	S. R. Marshall,	Wm. Sellers,
H. R. Heyl,	Henry Pemberton, Jr.,	H. W. Spangler.
H. W. Jayne,	C. E. Ronaldson,	

Members of the *Committee on Science and the Arts* (to serve three years) :

John E. Codman,	H. R. Heyl,	Henry Pemberton, Jr.,
Thos. P. Conard,	Fred. E. Ives,	Thomas Shaw,
C. B. Dudley,	W. M. McAllister,	L. H. Spellier,
Wm. C. Head,	Wm. D. Marks,	S. P. Sadtler,
C. J. Hexamer,	Philip Pistor,	T. C. Search.

The president appointed the following committees for the year 1891, viz :

<i>Library.</i>	<i>Minerals.</i>	<i>Models.</i>
J. Howard Gibson,	Clarence S. Bement,	Wm. C. Head,
Carl Hering,	Lee K. Frankel,	John H. Cooper,
F. L. Garrison,	Persifor Frazer,	Wm. M. McAllister,
Geo. A. Koenig,	F. A. Genth,	L. L. Cheney,
S. H. Needles,	Edwin J. Houston,	N. H. Edgerton,
Isaac Norris, Jr.,	George A. Koenig,	Philip Pistor,
H. W. Spangler,	Otto Lüthy,	Tinius Olsen,
Wm. P. Tatham,	E. F. Moody,	M. R. Mucklé, Jr.,
John C. Trautwine, Jr.,	Theo. D. Rand.	John J. Weaver,
Lewis S. Ware.	Wm. H. Wahl.	W. N. Jennings.
<i>Arts and Manufactures.</i>	<i>Meteorology.</i>	<i>Meetings.</i>
J. Sellers Bancroft,	Henry Crew,	Geo. V. Cresson,
C. J. Hexamer,	Charles M. Cresson,	G. M. Eldridge,
Cyrus Chambers, Jr.,	Edwin J. Houston,	Spencer Fullerton,
John Haug,	Isaac Norris, Jr.,	Henry R. Heyl,
Wm. B. Le Van,	Alex. E. Outerbridge, Jr.,	H. W. Jayne,
C. C. Newton,	H. Pemberton, Jr.,	Washington Jones,
Robt. Grimshaw,	J. S. W. Phillips,	Sam'l R. Marshall,
E. Alex. Scott,	M. B. Snyder,	Chas. E. Ronaldson,
Thomas Shaw,	Wm. P. Tatham,	Wm. H. Thorne,
D. E. Crosby.	H. Wiley Thomas.	S. L. Wiegand.

Adjourned.

WM. H. WAHL, *Secretary*.



# JOURNAL

OF THE

# FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

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VOL. CXXXI.

MARCH, 1891.

No. 3

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THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the journal

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## THE SYSTEM OF THE INTERIOR CONDUIT AND INSULATION COMPANY.

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*[Report of the Committee on Science and the Arts.]*

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[No. 1,485.]

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, December 31, 1890.

The Sub-Committee of the Committee on Science and the Arts, constituted by the Franklin Institute of the state of Pennsylvania, to whom was referred, for examination,

THE INTERIOR CONDUIT AND INSULATION CO.'S SYSTEM, respectfully report that they have carefully examined the same and report on it as follows:

The system is the subject of letters-patent of the United States:

No. 176,784, May 2, 1876. No. 359,726, Mar. 22, 1887.

No. 343,087, June 1, 1886. No. 401,498, April 16, 1889.

No. 354,320, Dec. 14, 1886. No. 435,897, Sept. 2, 1890.

VOL. CXXXI.

No. 435,898, Sept. 2, 1890.

No. 441,839, Dec. 2, 1890.

No. 441,835, Dec. 2, 1890.

No. 441,840, Dec. 2, 1890.

No. 441,836, Dec. 2, 1890.

No. 441,845, Dec. 2, 1890.

No. 441,837, Dec. 2, 1890.

No. 441,846, Dec. 2, 1890.

No. 441,838, Dec. 2, 1890.

No. 441,870, Dec. 2, 1890.

No. 441,090, Dec. 2, 1890.

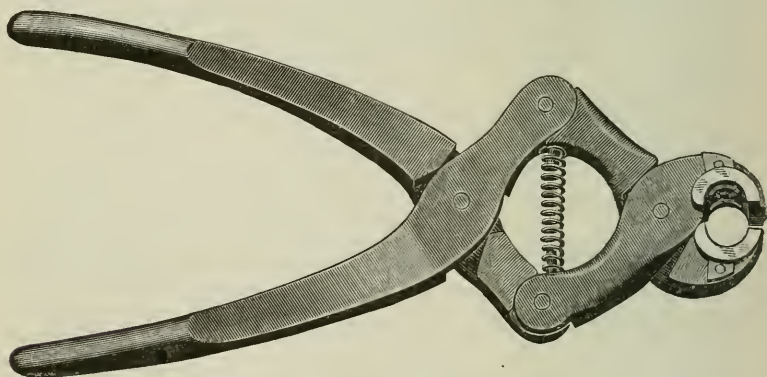
The purpose of the invention is to provide a means for running electric light wires in and through buildings and



New coupling.

underground. As it differs essentially from the usual system of running such wires, it may be said to be to a certain extent a new departure, rather than an improvement on the usual.

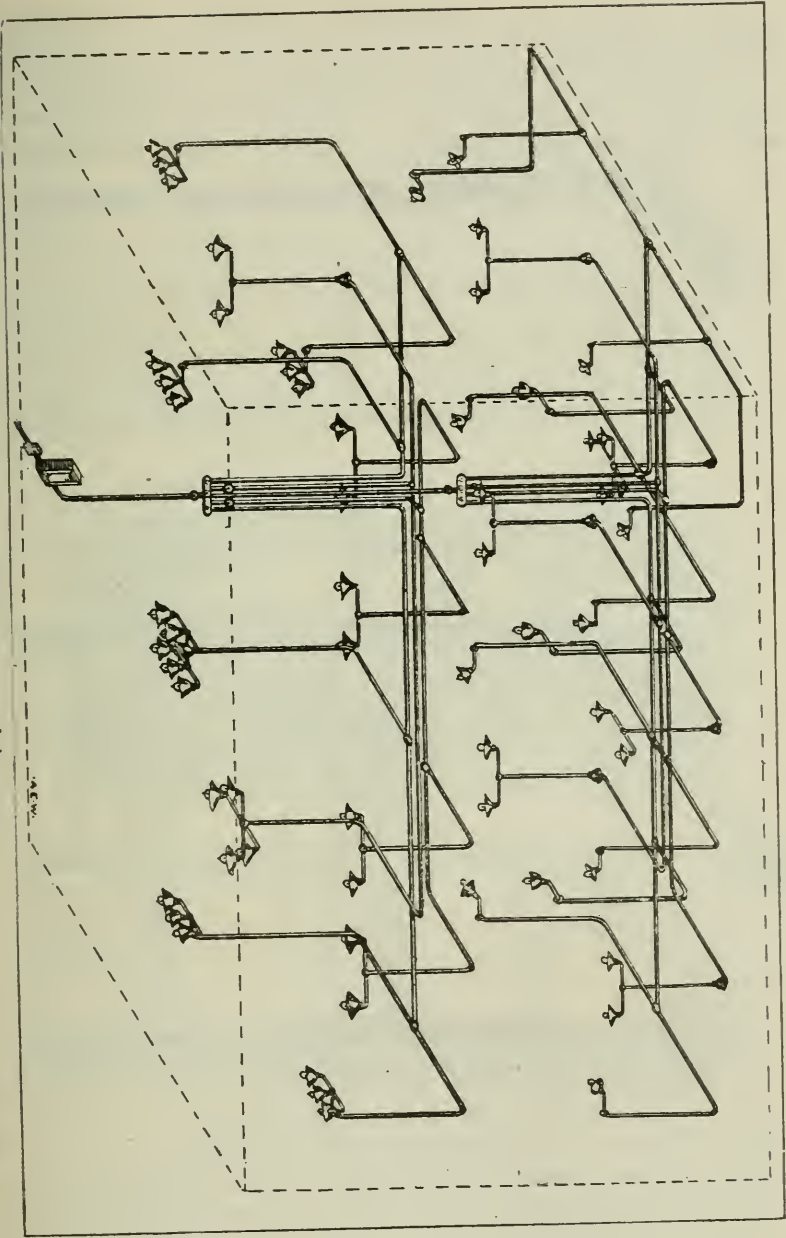
In general, it consists of a system of continuous tubes of insulating material, which are placed throughout a building, into which tubes the wires are afterwards drawn. The



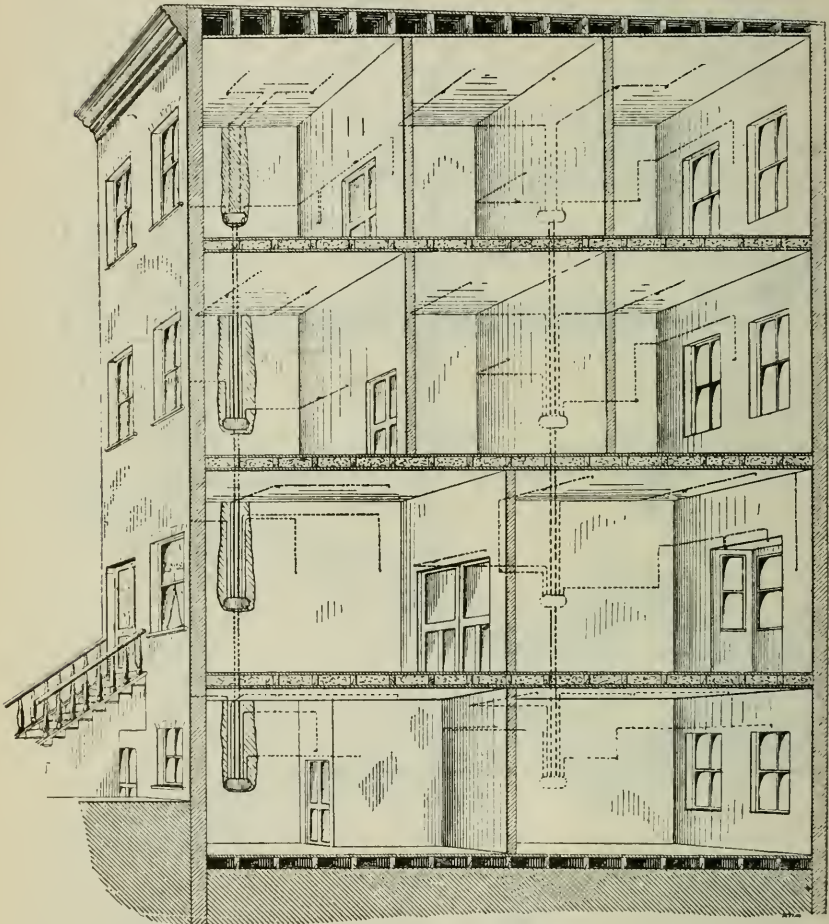
Coupling tool.

wires are thereby insulated electrically from the materials of the building, such as plaster, wood or stone-work, gas and water pipes, etc., while the wood-work of the building is also protected from being ignited by the accidental heating or short-circuiting of the wires themselves, thus protecting the building against liability to fires arising from such accidents.

Isometric perspective of conduit system.



The following is a description. The tubes are made of paper or similar material treated and soaked in a very hot bath of  $460^{\circ}$  F. of bituminous compounds. They are made in various sizes, ranging from one-quarter inch to one and one-half inches in inside diameter. They are hard, strong and

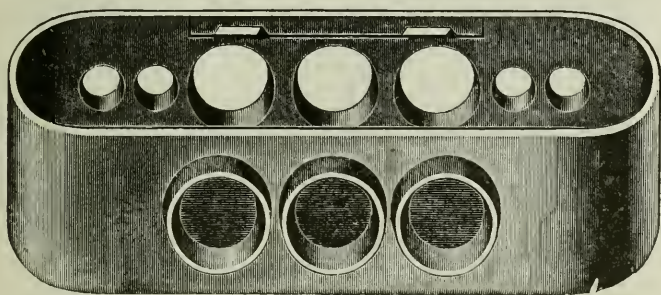


Conduit system in four-story building.

tough. They are made fire-proof, when necessary, by a coating of fire-proof paint on the outside which protects them from ignition by flames from the outside, and also prevents them from spreading a fire by carrying the flames; they cannot be ignited from the inside for want of sufficient air to support



combustion. They are therefore, in this sense, fire-proof, although made of inflammable material. They are claimed to be water-proof except where the tubes have been cut, as at joints. In cases where they must be absolute water-proof they are incased in a lead pipe. These tubes are secured to the wall of the building by a simple brass clip. They may be run in under or over the plaster. In the latter case they are finished externally so as to accord with the general finish of the wood-work or other ornamental work in the room. At corners of bends, the tubes are connected with butt joints to knee pieces, the joint being enclosed in a sleeve. These tubes are laid from the centre of distribution of a building to the outside of the fixtures. The centre of



Junction box.

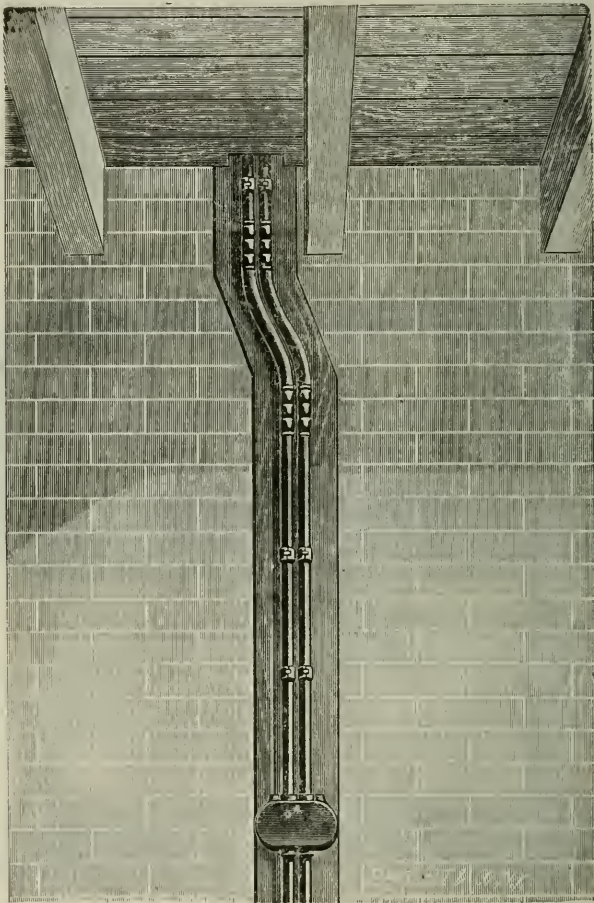
distribution is so chosen that it is within about thirty-five feet from the outlets. In underground work a number of these tubes are enclosed in an iron pipe, or cast-iron boxing, filled with an insulating compound.

The wires are afterwards drawn into the tubes by means of a "fishing" or "pilot" line. In case there are too many bends, a junction box is inserted to provide an opening to facilitate the drawing in of the wires. Five bends is given by the company as a limit through which a wire can be drawn. For large mains there is one tube for the positive and one for the negative main, but for all the smaller branches the two wires are run in the same tubes. These wires, whether double or single, have merely a light textile insulation, in



place of the usual expensive fire and water-proof coverings. That no danger arises from using such simple insulation was shown by the committee by experiments described below.

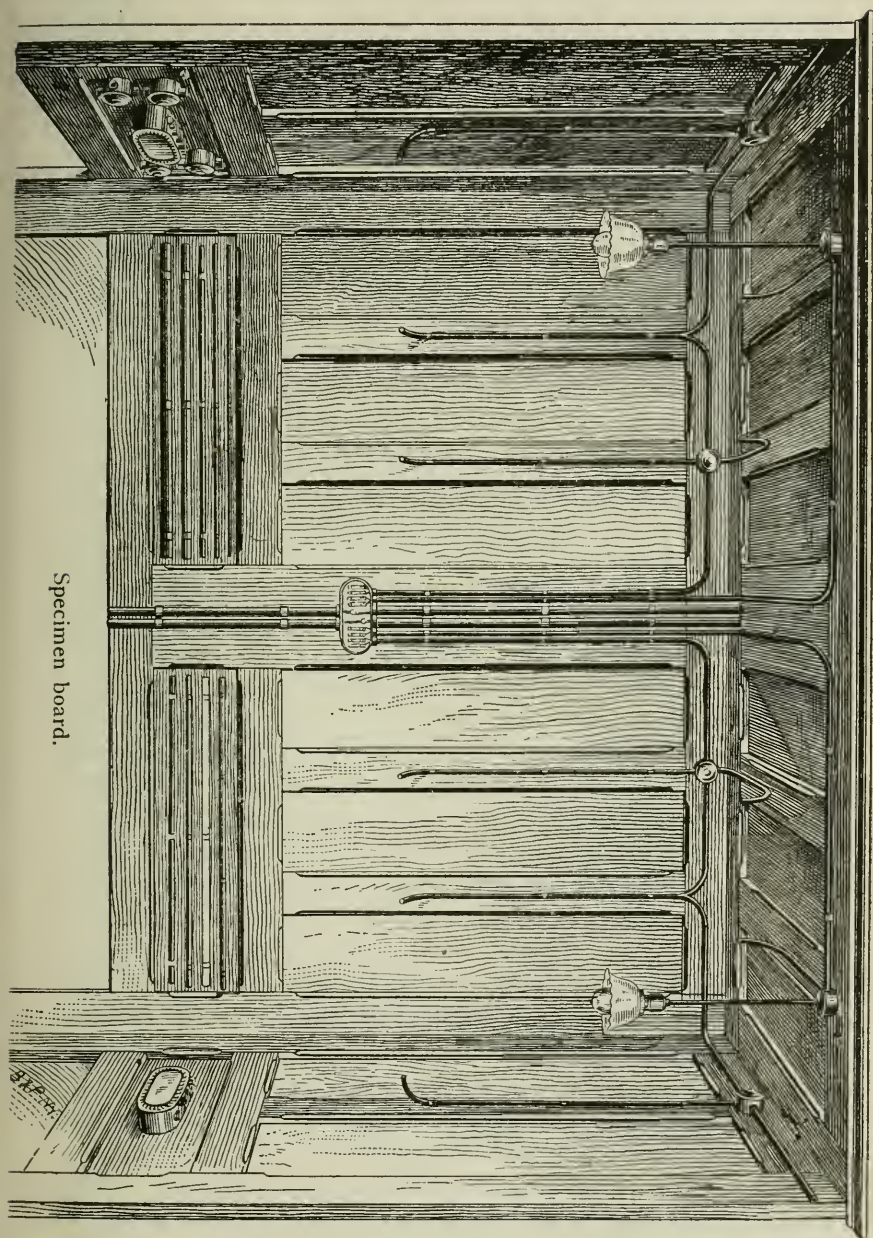
Among the merits of the invention are the following: It affords a more thorough insulation of the wires from the



Wall with risers.

building and protects the wires themselves from injury from external sources, such as moisture, abrasion, or other mechanical injury. It protects the building from fire arising from overheated wires, and thereby overcomes a great

Specimen board.



source of danger. It protects a building against fires arising from imperfect workmanship in running the wires, as loose joints, for instance. As the tubes are independent to a great extent, of the particular system of distribution to be used, it permits them to be laid when the building is constructed, the wires for any particular system being drawn into them when required. This overcomes an important objection to the usual system of the laying of the wires themselves into new buildings before it is known what system of distribution may be subsequently introduced. It also permits that the wires may be withdrawn and renewed in case they have been burnt out, or they may be replaced by large ones when it is desired to increase the number of lights at any subsequent time. The underground system was not exhibited to this committee, and is therefore not included in the report.

The following experiments were shown to the committee: A leak was forced between two wires, run, as is usual, on a wooden moulding, two and one-half inches apart, the leak was made, moistening the moulding with a solution of soda. The leakage was too small to actuate the safety fuses, but was large enough to char and set fire to the wood. The wires were placed close together, as in the system of tubes and the leak was produced as before. The leakage was so great that the safety fuses acted immediately, before any charring or ignition of the wood had time to take place. In another experiment a wire was secured to a piece of wood, and a continuation of the same wire was passed through the tubes; the whole wire was then overloaded so as to cause it to heat. The wood was set on fire, but the tube was not hot enough to do any harm, although the insulation around the wire itself had been completely consumed by the heat. Other experiments were made showing that a short circuit in a tube would not start a fire, while a similar short circuit in the open air or moulding is very apt to be a source of fire.

The question of the scope or extent of the patents was not considered by the committee.

Your committee endorse the merits of the above invention and add that in their opinion it is an improvement of importance and value. In view of these facts they recom-



mend the award of the John Scott Legacy Medal and Premium.

CARL HERING, *Chairman*,      RICHARD W. DAVIDS,  
T. CARPENTER SMITH,      E. A. SCOTT,  
WM. McDEVITT,

*Adopted, February 4, 1891.*

S. L. WIEGAND,

*Chairman of the Committee on Science and the Arts.*

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THE SYSTEM OF HOUSE AND UNDERGROUND  
WIRING OF THE INTERIOR CONDUIT AND  
INSULATION COMPANY.

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BY E. H. JOHNSON, New York.

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[*A Paper read before the Franklin Institute, December 17, 1890.*]

JOSEPH M. WILSON, president, in the chair.

MR. JOHNSON—MR. PRESIDENT AND MEMBERS OF THE  
INSTITUTE:

Having been identified with the new industry of incandescent electric lighting, and more particularly with the practical work of distributing the electric current therefor, since its inception, I have been made aware of the requirements of the problems of safety, convenience and economy in a pronounced way, and in the system which I am honored in being requested to bring to your attention to-night, I claim to have fully incorporated these essential factors.

Briefly described, the system is nothing more or less than a well worked-out scheme of supplanting or supplementing the former practice of attaching the insulation to the wire itself, by a system of insulated raceways, into which to draw the wires, and which supplies in itself a degree of high and durable insulation not attainable by the old methods, while supplying in addition the highly important (I might almost say imperative) adjuncts of accessibility.

In the matter of safety, I have only to call your attention to the fact that it is practically impossible to main-

tain a flame within the circumscribed limits of a tube scarcely greater in diameter than the material ignited within it; such an enclosure becomes a snuffer, and this quality of suffocating combustion is independent of the inflammability of the material of which the snuffer may be composed. No elaborate description of the system is required. The sample of wiring which stands before you tells its own story on the instant, and the experiments which my assistant and associate inventor, Mr. Greenfield, will now make, will demonstrate its utility better than any verbal explanation. Before proceeding to such demonstration, however, I will make mention of one or two points which an experience of a year in promoting this system has developed.

(1) You will readily perceive that if this means of insulating wires for interior or underground purposes should in practice prove all-sufficient, the very large interests now invested in the wire-insulating industries would imagine itself seriously jeopardized, and the new method would encounter the open and severe attack of such interest. This attack has been made, and while I have not claimed for the system that it is a complete substitute for all other insulations, I have, by virtue of the very efforts that have been made to depreciate it, been led to develop it on lines which will speedily enable it to fulfil such conditions absolutely.

In illustration, certain rubber insulation interests in the city of Boston, undertook to prove to interested authorities that the tube would burn, not by virtue of heat emanating from the wires within it, but by fires originating from other and external causes; and this demonstration was made quietly and without my knowledge. Upon learning the facts, I at once called attention to the fact that no claim of non-combustibility had ever been made for it. That such was, in fact, not an essential requisite, nevertheless, I then undertook to provide a special tube which might be employed in certain cases where this feature was important. A sample of this tube will be shown you to-night.

(2) It has been alleged also that this tube disintegrates



in time in the presence of moisture. This criticism was very properly made against the first tube we placed on the market, but when the new tube completely met it, the criticism was not abandoned but was kept alive by the exhibitions of the early samples. When, however, in the course of time, I made this contention untenable, it was narrowed down to the statement that the tube would disintegrate in cement. Upon investigation I found this to be true and endeavored at once to correct it if possible. I am glad to say that this most difficult of all problems has now been satisfactorily solved.

(3) It has been asserted that its cost, in consequence of its not being a complete substitute for other insulation, is simply an additional charge on an installation and one that brings no sufficient compensations. To this I reply that the cost of an installation by this method need be no greater than that of any other method approved by the better class of underwriter inspectors. Not so great, in fact, as some methods. The difficulty is that too much work is done, where reliance is placed wholly on the insulation of the wire itself. In fact, notwithstanding the universal knowledge that no insulated wire exists which does not need to be physically protected, many installations are thus made to-day and pass the inspectors, where owners and architects, however, take an intelligent interest in the subject and insist upon superior work. This system can be and is installed at prices that compete with others.

Furthermore, the details of its application are being cheapened and improved every day, and it is probably not even now too much to claim for the system that its opponents have caused it to be developed to a degree that will justify the contention that their wires may be safely employed in it and thus the tables be turned; that is to say, it may be installed more cheaply than any other, because the costly insulation of wires by its use are dispensed with.

I venture, in conclusion, to submit a statement and description of a new underground conduit system, which is more or less the outgrowth of the interior conduit system just described, and is the result of a series of costly and

elaborate experiments to determine the practical requirements of underground electric conductors. The system is not unlike the interior system, in that it consists of a series of highly insulating, water-proof tubes, which form individual channels or raceways for the reception of whatsoever electrical conductors may be employed, thus providing for the drawing in, or withdrawal of, conductors at any time, rendering them as accessible for renewal and repairs as if placed on the surface itself.

In its physical characteristics it differs from the ordinary iron, wood, cement and other conduits, in that while they only contemplate ducts of uniform and large diameter, each accommodating a considerable number of wires, these conduits provide an individual duct of suitable dimensions for each and every wire or cable employed. In the important element of insulation, however, it differs radically. All conduits hitherto employed practically ignore the problem of insulation, thus compelling the use of costly and cumbersome insulations upon the wires themselves, whereas this system provides in the conduit itself without special investment therefor an insulation ample for all purposes. The extra charge, direct and indirect, incurred for the necessarily expensive insulations and metallic protections of such insulated cables and for handling their greater bulk and weight, will, in any plant of material size, amount to a sum more than sufficient to pay the entire cost of this conduit system.

This conduit may consist of any desired number of individual tubes or ducts, continuous from one man-hole to another, no joints or couplings being employed to unite the several sections of the individual ducts, and thus a fruitful cause of leakage and consequent deterioration is eliminated. The method of converting the tube sections into continuous unbroken and unjointed ducts constitutes one of the important elements of our system.

The general method is as follows :

A series of tubes of any given number, length and diameter is racked in a suitable frame so as to be held at a uniform distance apart. The segregated bundle thus

formed is then pushed into a cast-iron pipe of a diameter slightly greater than that of the bundle of tubes, but of exactly the same length. A jig is then attached to the end to secure a uniform terminal location of the ducts, and the whole is then immersed in a bath of special insulating compound maintained at a high temperature. When withdrawn from this bath, only that portion of the compound which has entered the interior of the tubes is allowed to escape, all that portion which has filled the waste interstices is imprisoned, and subsequently becomes solidified, thus presenting a substantial body composed only of a high insulating material, throughout which are the clear, straight and smooth duct interiors; these completed sectional bodies are for convenience of handling manufactured in ten-foot lengths, and in this form are ready for laying. Realizing, however, that all attempts at hermetically uniting the multiple ducts of such sectional systems have proven utter and disastrous failures, we have evolved a method of laying these conduits, which converts these sectional units into continuous ducts having practically no junctions, certainly none of inclusive cross section, or contiguous coincidence. The manner in which this is effected is as follows:

In addition to the consolidated section above described, we employ individual tubes the length of which is the same as the consolidated ducts, the external diameter of which, however, is slightly less than the internal diameter of the consolidated ducts and the internal diameter of which is just what is called for by the requirements of the service for which they are intended. These individual tubes are coated at the moment of laying with an insulating compound or cement, and are then telescoped into the consolidated ducts to the depth of just one-half their length in each section. The abutting ends of the exterior or consolidated tubes of contiguous sections are, of course, mid-way of the general containing pipes, each and every duct thus telescopically formed of the two complete tubes and cemented with a superior insulating compound, becomes, therefore, to all intents and purposes as perfect in its continuity as if manufactured at the factory in one unbroken

length. The general containing pipes are united by a special sleeve or coupling box, which is thoroughly filled with insulating compound and further sealed by the insertion of a sheet-lead gasket between the sleeve or box and the abutting ends of the pipe.

From man-hole to man-hole, we thus provide a system which in its outer material form and appearance is identical with that method, which fifty years of practical experience has evolved as best practice in the distribution of water, gas, steam, etc. Cast iron having become the accepted economical and durable channel for the distribution of energy in all its other forms, why not for the distribution of electricity? The answer to this question has hitherto been found in the absence of suitable provision for the continuity of insulation in each and every one of the separate ducts demanded by the peculiar requirements of electrical distribution, viz: That the aggregate volume traversing a given channel must consist of separately maintained minor units or volumes, each of which possesses some special characteristic of pressure or other function differentiating it from its neighbor. This problem of absolutely reliable insulating continuity is now solved for the first time by our telescopically continuous insulated ducts.

*Man-holes.*—Having applied to electrical channel distribution the best practice evolved by other and older industries, and having only supplemented these with new devices where the special requirements of electric distribution demanded, we have proceeded to analyze the terminal requirements of electrical conductors and, as far as possible, have endeavored to apply thereto that method of practice which experience has demonstrated to be most effective. The “man-hole” is practically a creation of the electrical industry; we are therefore limited in any consideration of its problems to electrical practices; fortunately, however, the man-hole is an indispensable adjunct of all electrical systems, hence, by virtue of the extraordinary development of the electrical industry, we become possessed of a vast deal of practical experience from which to obtain useful data. An examination of these data develops the fact that the



demand is for a man-hole which may be left open with impunity. This means simply that the conductors passing through it and the ducts entering it must be considered, in so far as their treatment is concerned, as though permanently terminating in a cesspool. We have attacked the problem from this point of view.

The man-hole as we have finally designed it is simply a construction of brick and carefully cemented to protect it as far as possible from the exudations of the soil, for the sake of cleanliness. The section of the general containing pipe entering the man-hole is expanded in a funnel form to about double its normal diameter, for the purpose of separating, and, therefore, providing a greater space around the duct ends for more perfect manipulation. The end of this funnel is provided with an iron head, on the face of which are short pipe projections corresponding to the number of ducts, and into each of which a duct end terminates. These iron nipples are threaded on the outside for the reception of a metallic cap or coupling. When the conductor has been drawn into the duct a section of lead pipe with an insulated lining is pushed over the end of the conductors, and a short distance into the duct itself; a coupling is then screwed onto the cap and over the lead-encased conductor and so firmly screwed home as practically to consolidate the lead sheathing and iron nipples, and thus hermetically seal the end of the duct. The corresponding end of the conductor on the opposite side of the man-hole having been treated in the same way, it now only remains to join the broken sections of the conductors and their lead sheathing, hang the completed conductor upon its proper rack at the side of the man-hole, and proceed to treat each of the other conductors in a like manner. When all are so treated, and any unoccupied ducts securely sealed, the man-hole itself may be left open with impunity.

It will be observed that since each and every conductor is treated at the man-hole as flexible cable, and hung upon a rack at its side, the centre of the man-hole remains unobstructed, and perfect freedom is afforded the workmen who may need to get at any particular conductor for purpose of testing, repairs or lateral tapping.



Lamp-posts, feeder-posts, or other branch taps are provided, and are so constructed as to be nothing more or less than minor extensions of the system in its every feature. They are iron pipes filled with our insulating compound and jointed to the conduit proper by suitably constructed elbow extensions, thus providing for the drawing in of the conductors from the nearest man-hole to the top or extreme terminal of the post or other branch.

In conclusion, I may say that we feel free to express our conviction that in point of first cost, durability, flexibility, insulation, cleanliness, economy of maintenance, economy of space and general adaptability, no other combination of underground conduit and conductors can even approximately equal this system.

I thank you, Mr. President and gentlemen, for your courteous attention.

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## THE CONTINUOUS GIRDER—VARIABLE MOMENT OF INERTIA—FIXED POINTS—GRAPHIC METHOD.

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BY C. H. LINDENBERGER.

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Those who are familiar with graphic methods would scarcely like to lose the sense of assured satisfaction that comes from proving the agreement of a graphic calculation with an analytical one, even though the latter may not be so very difficult. This is the case in the majority of instances, but there is this advantage that an increased difficulty in the analytical calculation seldom means a corresponding difficulty in the graphical method. Hence a graphic theory is always valuable as a means of solution.

The theory of solution by "fixed points," so far as it relates to "constant moment of inertia," has been dealt with in this journal for December, 1888, and November, 1889. It remains to extend it to "variable moment of inertia," and, to make the theory general, to extend it to the case in which the coefficients of the unknown quantities are different in each equation.

Thus, for instance, let there be a series of equations like this to be solved:

$$\begin{aligned} Q_2 M_2 + R_2 M_3 &= Y_2 \\ U_2 M_2 + Q_3 M_3 + R_3 M_4 &= Y_3 \\ \text{etc., etc., etc., etc.} \\ U_{s-2} M_{s-2} + Q_{s-1} M_{s-1} + R_{s-1} M_s &= Y_{s-1} \\ U_{s-1} M_{s-1} + Q_s M_s &= Y_s \end{aligned} \quad (1)$$

There are  $(s-1)$  of these equations, and also  $(s-1)$  unknown quantities denoted by  $M$  with their proper subscripts. All the other quantities are known.

Hence the problem is soluble by ordinary algebra, and is, in fact, in all of its phases nothing but algebra, though in practice the continuous girder furnishes the problem.

Multiply the first equation by  $D_2$ , the second by  $D_3$ , etc., add and arrange as coefficients of the unknown quantities, and we have

$$\left\{ \begin{aligned} &M_2 (D_2 Q_2 + D_3 U_2) - D_2 Y_2 \\ &+ M_3 (D_2 R_2 + D_3 Q_3 + D_4 U_3) - D_3 Y_3 \\ &\quad \text{etc., etc., etc., etc.} \\ &+ M_{s-1} (D_{s-2} R_{s-2} + D_{s-1} Q_{s-1} + D_s U_{s-1}) - D_{s-1} Y_{s-1} \\ &+ M_s (D_{s-1} R_{s-1} - D_s Q_s) - D_s Y_s \end{aligned} \right\} = 0 \quad (2)$$

Assume that the coefficient of  $M_q = Z_q$ , and that of all the others  $= 0$ , and we have

$$M_q = \frac{D_2 Y_2 + D_3 Y_3 + \text{etc.,} + D_{s-1} Y_{s-1} + D_s Y_s}{Z_q} \quad (3)$$

$$D_{q-1} R_{q-1} + D_q Q_q + D_{q+1} U_q = Z_q \quad (4)$$

By reason of this assumption we have here  $(s-1)$  equations and  $(s-1)$  unknown quantities of the type  $D$  and  $Z_q$ . If we assume that  $D_2 = 1$ , all the rest of these quantities become determinate.

It is better, however, to ascertain their values differently.

Assume the following equations:

$$\begin{aligned} c_1 R_1 + c_2 Q_2 + c_3 U_2 &= 0 \\ c_2 R_2 + c_3 Q_3 + c_4 U_3 &= 0 \\ \text{etc., etc., etc., etc.} \end{aligned} \quad (5)$$

$$\begin{aligned} c_{s-2} R_{s-2} + c_{s-1} Q_{s-1} + c_s U_{s-1} &= 0 \\ c_{s-1} R_{s-1} + c_s Q_s + c_{s+1} U_s &= 0 \end{aligned}$$

There are  $(s-1)$  of these equations, and by assuming that  $c_1 = 0$ ,  $c_2 = 1$  all the unknown quantities of the type  $c$  become determinate.

Write  $d$  for  $c$  in the above equations and assume that  $d_{s+1} = 0$ ,  $d_s = 1$  then all the unknown quantities of the type  $d$  become determinate.

By examination of the series of equations (5) it is evident that the equations for the determination of  $D_n$ , where  $n$  is equal to or less than  $q$  have the same form as those by which  $c_n$  is found, and, moreover,  $D_2 = c_2 = 1$ . Hence

$$D_n = c_n \quad \text{for } n = \text{or } < q \quad (6)$$

These equations, as modified by the substitution of  $d$  for  $c$ , also show a series of the same form for the determination of

$$\frac{D_n}{D_s} \text{ and } \frac{d_n}{d_s}$$

if  $n$  is either equal or greater than  $q$ . Hence

$$\frac{D_n}{D_s} = \frac{d_n}{d_s} = d_n \text{ since } d_s = 1$$

$$\text{for } n = \text{or } > q$$

and making  $n = q$ , equation (6)

$$D_s = \frac{c_q}{d_q}$$

whence

$$D_n = \frac{c_q}{d_q} d_n \quad (7)$$

$$\text{for } n = \text{or } > q$$

These values in equation (4) give us

$$c_{q-1} R_{q-1} + c_q Q_q + \frac{c_q}{d_q} d_{q+1} U_q = Z_q \quad (8)$$

and we also have

$$c_{q-1} R_{q-1} + c_q Q_q + c_{q+1} U_q = 0 \quad (9)$$

$$d_{q-1} R_{q-1} + d_q Q_q + d_{q+1} U_q = 0 \quad (10)$$

Equation (8) becomes from equation (9)

$$(c_q d_{q+1} - c_{q+1} d_q) U_q = d_q Z_q \quad (11)$$

Multiply equation (9) by  $d_q$  and equation (10) by  $c_q$  and subtract the latter, and we have

$$(c_{q-1} d_q - c_q d_{q-1}) R_{q-1} - (c_q d_{q+1} - c_{q+1} d_q) U_q = 0$$

which may be written

$$\frac{R_{q-1}}{U_{q-1}} (c_{q-1} d_q - c_q d_{q-1}) U_{q-1} = (c_q d_{q+1} - c_{q+1} d_q) U_q$$

or from equation (11)

$$\frac{R_{q-1}}{U_{q-1}} d_{q-1} Z_{q-1} = d_q Z_q \quad (12)$$

It thus appears that  $d_q Z_q$  is not a constant quantity, for by hypothesis  $R_{q-1}$  is different from  $U_{q-1}$ . If, however, this were not the case; that is, if the rule were universal that  $U_n = R_n$ , then we would have

$$d_q Z_q = d_{q-1} Z_{q-1} = d_s Z_s = Z_s \quad (13)$$

This is the case with the usual form of equations for "constant moment of inertia."

The original equations may be transformed so that new coefficients are evolved in which this rule would hold good; that is, the new value of  $U_n$  will be equal to the new value of  $R_n$ . This is done in the following manner:

Multiply the first of the original equations (1) by  $s_1 s_2$ , the second by  $s_2 s_3$ , etc., and assume the following equations:

$$s_2 s_3 U_2 = s_1 s_2 R_2, \\ \text{etc., etc., etc.}$$

$$s_{s-1} s_s U_{s-1} = s_{s-2} s_{s-1} R_{s-1}$$

The number of these equations is  $(s-2)$ . Hence, if we assume the value of  $s_1$  and  $s_2$ , we can find the value of the other unknown quantities of the type  $s$ , and thence the value of the new coefficients.

This, however, is not necessary in the present instance, for the original equation for the continuous girder of vari-

able moment of inertia has been shown by the previous article to take the following form :

$$M_{r-1} L_{r-1} + M_r (T_r + G_{r-1}) + M_{r+1} L_r = Y_r \quad (14)$$

where

$$Y_r = A_r + B_{r-1} - \left( \frac{h_r - h_{r+1}}{l_r} + \frac{h_r - h_{r-1}}{l_{r-1}} \right) \quad (15)$$

$M$  is the moment at the support denoted by its subscript and  $h_r$  is the vertical ordinate of that support. The quantities  $L_r$   $G_r$   $T_r$   $A_r$  and  $B_r$  have been defined in this *Journal*, January, 1891, but the definition is too tedious to be given here.  $l_r$  is the length of the  $r^{\text{th}}$  span.

Equations (9), (10), (11) and (13) become, respectively substituting  $r$  for  $q$ ,

$$c_{r-1} L_{r-1} + c_r (T_r + G_{r-1}) + c_{r+1} L_r = 0 \quad (9a)$$

$$d_{r-1} L_{r-1} + d_r (T_r + G_{r-1}) + d_{r+1} L_r = 0 \quad (10a)$$

$$(c_r d_{r+1} - c_{r+1} d_r) L_r = d_r Z_r = Z_s \quad (11a)$$

$$d_r Z_r = d_{r-1} Z_{r-1} = d_s Z_s = Z_s \quad (13a)$$

and from equations (6) and (7) and (13a) we deduce

$$M_r = \frac{d_r (c_2 Y_2 + c_3 Y_3 \text{ etc.} + c_r Y_r) + c_r (d_{r+1} Y_{r+1} + \text{etc.} + d_s Y_s)}{Z_s} \quad (16)$$

which is the same form as that given in this *Journal* for December, 1888.

$$Z_s = c_{s-1} L_{s-1} + c_s (T_s + G_{s-1}) \quad (17)$$

It should be remarked that the girder has  $s$  spans, and therefore  $(s+1)$  supports. We cannot therefore in equation (14) have either  $r = 1$  or  $r = s + 1$ . An obvious reason is, among others, that we should only have as many equations as there are unknown moments to be determined since the moments at the end supports  $= 0$ . Of course, this applies to equation (14) only, and not to the rest.

For the purpose of a graphical solution let us assume that in the  $r^{\text{th}}$  span there are two "fixed points," whose dis-



tance from the  $r^{\text{th}}$  support is given by the following equations:

$$i_r = \frac{c_r l_r}{c_r - c_{r+1}} \quad (18)$$

$$I_r = \frac{d_r l_r}{d_r - d_{r+1}} \quad (19)$$

These points have been called "fixed inflection points," or "points of zero moment." Now, while either of these names are not misnomers they have all the effect of it.

They were originally so called because they are in certain cases, inflection points or points of zero moment for loads or disturbances of level in other spans, but the names tend to create the impression that they are *always* such under all circumstances, even among those who ought to know better. I prefer, therefore, simply to call them "fixed points."

By the aid of equations (18), (19) and (11a), we obtain

$$Z_s = c_r d_r \left( \frac{I_r - i_r}{I_r i_r} \right) L_r l_r \quad (20)$$

I will now show how these fixed points are obtained for either constant or variable moment of inertia. In *Fig. 1*,

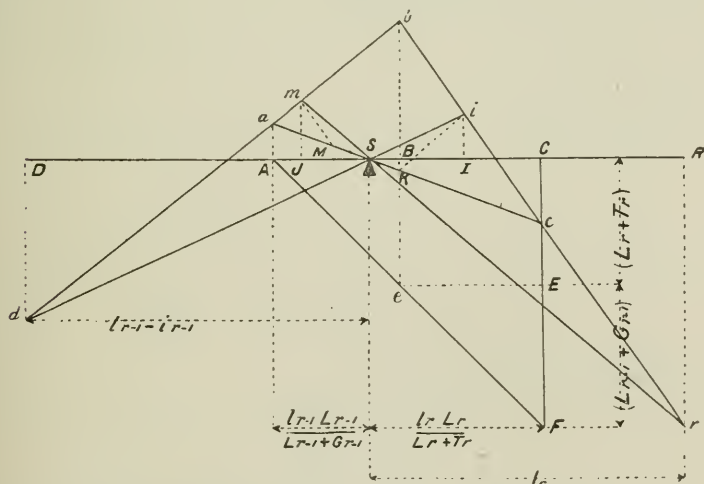


FIG. 1.

Let  $S$  be the  $r^{\text{th}}$  support.

$$\text{Let } \overline{DS} = l_{r-1} - i_{r-1} = -\frac{c_r l_r}{c_{r-1} - c_r}$$

$$\text{Let } \overline{SR} = I_r = \frac{d_r l_r}{d_r - d_{r+1}}$$

$$\overline{AS} = \frac{l_{r-1} L_{r-1}}{L_{r-1} + G_{r-1}}$$

$$\overline{SC} = \frac{l_r L_r}{L_r + T_r}$$

$$\overline{CE} = \text{or proportional to } (L_r + T_r)$$

$$\overline{EF} = \text{ " " } (L_{r-1} + G_{r-1})$$

The lengths  $\overline{AS}$  and  $\overline{SC}$  have been graphically obtained in the previous article, and are found from the intersection of the cross lines formed by the prolongation of the end sides of the equilibrium polygons required for the graphical calculation of  $L_r$ ,  $T_r$  and  $G_r$ .

Draw vertical lines through all these points. Take  $\overline{Dd}$  any convenient length and  $\overline{bd}$  any convenient slope, and this will determine the length of  $\overline{Rr}$ . Complete the figure. The lines  $\overline{ik}$  and  $\overline{mM}$  are not drawn in practice, but are necessary for the demonstration.

The former is parallel to  $\overline{bd}$  and the latter to  $\overline{br}$ . Then  $\overline{SI} = i_r$  and  $\overline{SJ} = l_{r-1} - I_{r-1}$ .

First the proof as to  $\overline{SI}$ .

From the similar triangles  $\overline{ikc}$  and  $\overline{abc}$ , we have

$$\overline{ik} = \overline{ab} \times \frac{\overline{ic}}{\overline{bc}} = ab \times \frac{\overline{IC}}{\overline{BC}}$$

Also from the similar triangles  $\overline{ikS}$  and  $\overline{dSa}$  we have

$$\overline{ik} = \overline{da} \times \frac{\overline{Si}}{\overline{dS}} = \overline{da} \times \frac{\overline{SI}}{\overline{DS}}$$

Equating these and making

$$\frac{\overline{ab}}{\overline{da}} = \frac{\overline{AB}}{\overline{DA}}$$

$$\frac{\overline{SI}}{\overline{DS}} = \frac{\overline{AB}}{\overline{DA}} \cdot \frac{\overline{IC}}{\overline{BC}} = \frac{\overline{AB}}{\overline{DA}} \left( \frac{\overline{SC} - \overline{SI}}{\overline{BC}} \right)$$

whence

$$\overline{SI}(\overline{DA} \cdot \overline{BC} + \overline{AB} \cdot \overline{DS}) = \overline{AB} \cdot \overline{SC} \cdot \overline{DS}$$

Now

$$\overline{DA} = \overline{DS} - \overline{AS}$$

and

$$\overline{BC}(\overline{DS} - \overline{AS}) + \overline{AB} \cdot \overline{DS} = \overline{DS} \cdot \overline{AC} - \overline{AS} \cdot \overline{BC}$$

whence

$$\overline{SI} = \frac{\overline{AB} \cdot \overline{SC} \cdot \overline{DS}}{\overline{DS} \cdot \overline{AC} - \overline{AS} \cdot \overline{BC}} = \frac{\overline{SC}}{\frac{\overline{AC}}{\overline{AB}} - \frac{\overline{BC}}{\overline{AB}} \cdot \frac{\overline{AS}}{\overline{DS}}}$$

$$= (\text{by similar triangles}) = \frac{\overline{SC}}{\frac{\overline{CF}}{\overline{CE}} - \frac{\overline{EF}}{\overline{CE}} \cdot \frac{\overline{AS}}{\overline{DS}}}$$

$$\left( \text{because } \frac{\overline{BC}}{\overline{AB}} = \frac{\overline{EF}}{\overline{CE}} \right)$$

$$= \frac{\overline{CE} \cdot \overline{SC}}{\overline{CF} - \frac{\overline{EF} \cdot \overline{AS}}{\overline{DS}}}$$

and substituting the values

$$\begin{aligned} \overline{SI} &= \frac{l_r L_r}{L_r + T_r + L_{r-1} + G_{r-1} + L_{r-1} l_{r-1} \frac{(c_{r-1} - c_r)}{c_r l_{r-1}}} \\ &= \frac{c_r L_r l_r}{c_r (L_r + T_r + L_{r-1} + G_{r-1}) + c_{r-1} L_{r-1} - c_r L_{r-1}} \end{aligned}$$

but

$$c_{r-1} L_{r-1} + c_r (T_r + G_{r-1}) = -c_{r+1} L_r$$

whence

$$\overline{SI} = \frac{c_r l_r}{c_r - c_{r+1}} = i_r \quad (21)$$

which was to be proved.

The next is the proof as to  $\overline{SJ}$ .

From the similar triangles  $\overline{m M a}$  and  $\overline{a r c}$  we have

$$\overline{m M} = \overline{b c} \cdot \frac{\overline{a m}}{\overline{a b}} = \overline{b c} \cdot \frac{\overline{A J}}{\overline{A B}}$$

Also from the similar triangles  $\overline{m M S}$  and  $\overline{r S c}$ , we have

$$\overline{m M} = \overline{c r} \cdot \frac{\overline{S m}}{\overline{S r}} = \overline{c r} \cdot \frac{\overline{S J}}{\overline{S R}}$$

Equating these and putting

$$\frac{\overline{b c}}{\overline{c r}} = \frac{\overline{B C}}{\overline{C R}}$$

we get

$$\frac{\overline{S J}}{\overline{S R}} = \frac{\overline{B C}}{\overline{C R}} \cdot \frac{\overline{A J}}{\overline{A B}} = \frac{\overline{B C}}{\overline{C R}} \left( \frac{\overline{A S} - \overline{S J}}{\overline{A B}} \right)$$

$$\overline{S J} (\overline{C R} \cdot \overline{A B} + \overline{B C} \cdot \overline{S R}) = \overline{B C} \cdot \overline{A S} \cdot \overline{S R}$$

Now

$$\overline{C R} = \overline{S R} - \overline{S C}$$

and

$$\overline{A B} (\overline{S R} - \overline{S C}) + \overline{S R} \cdot \overline{B C} = \overline{S R} \cdot \overline{A C} - \overline{A B} \cdot \overline{S C}$$

whence

$$\overline{S J} = \frac{\overline{B C} \cdot \overline{A S} \cdot \overline{S R}}{\overline{S R} \cdot \overline{A C} - \overline{A B} \cdot \overline{S C}} = \frac{\overline{A S}}{\frac{\overline{A C}}{\overline{B C}} - \frac{\overline{A B} \cdot \overline{S C}}{\overline{B C} \cdot \overline{S R}}}$$

$$(\text{by similar triangles}) = \frac{\overline{A S}}{\frac{\overline{C F}}{\overline{E F}} - \frac{\overline{C E}}{\overline{E F}} \cdot \frac{\overline{S C}}{\overline{S R}}}$$

$$= \frac{\overline{E F} \cdot \overline{A S}}{\overline{C F} - \overline{C E} \cdot \frac{\overline{S C}}{\overline{S R}}}$$

and by substituting the values

$$\begin{aligned} \overline{S J} &= \frac{l_{r-1} L_{r-1}}{L_r + T_r + L_{r-1} + G_{r-1} - l_r L_r} \cdot \frac{(d_r - d_{r+1})}{d_r l_r} \\ &= \frac{d_r l_{r-1} L_{r-1}}{d_r (L_r + T_r + L_{r-1} + G_{r-1}) - d_r L_r + d_{r+1} L_r} \end{aligned}$$

Now

$$d_r (T_r + G_{r-1}) + d_{r+1} L_r = -d_{r-1} L_{r-1}$$

whence

$$\bar{S} \bar{J} = \frac{d_r l_{r-1}}{d_r - d_{r-1}} = l_{r-1} - I_{r-1} \quad (22)$$

which was the solution sought.

The reader may possibly be puzzled to account for the assumption of  $l_{r-1} - i_{r-1}$  and  $I_r$ , but the explanation is easy.

We know from the equation that  $i_1 = 0$ , hence we can find  $i_2$ , then  $i_3$ , etc., to the last span. We also know from the equation that  $I_s = l_s$ , hence we can find  $I_{s-1}$ , then  $I_{s-2}$ , etc., back to the first span, and the operation is complete.

For constant moment of inertia

$$L_r = \frac{l_r}{\theta} \quad G_r = T_r = \frac{2 l_r}{\theta}$$

where  $\theta$  is six times the modulus of elasticity multiplied by the moment of inertia and is constant. Hence for this case we have

$$\bar{A} \bar{S} = \frac{l_{r-1}}{3} = \bar{B} \bar{C}$$

and

$$\bar{S} \bar{C} = \frac{l_r}{3} = \bar{A} \bar{B}$$

and the construction is simpler.

The next step is to use the definition of these fixed points to determine the moments at the supports. Now,  $A_r$  and  $B_r$  (see this *Journal*, January, 1891) are terms which depend on the load in the  $r^{\text{th}}$  span and for supports on a level and only this span loaded, we deduce

$$M_r = \frac{I_r i_r \frac{A_r}{L_r l_r} - i_r (l_r - I_r) \frac{B_r}{L_r l_r}}{I_r - i_r} \quad (23)$$

$$M_{r+1} = \frac{(l_r - i_r) (l_r - I_r) \frac{B_r}{L_r l_r} - i_r (l_r - I_r) \frac{A_r}{L_r l_r}}{I_r - i_r} \quad (24)$$



These equations are similar to those given in this *Journal* for November, 1889, except that we now have

$$\frac{A_r}{L_r l_r} \text{ and } \frac{B_r}{L_r l_r}$$

If we call the first one  $= A'_r$  and the second  $= B'_r$ , they will be of the same form and for constant moment of inertia will have the value given there. Hence the diagram given in that article could be used, but the present one (*Fig. 2*) is simpler and less liable to graphical error.

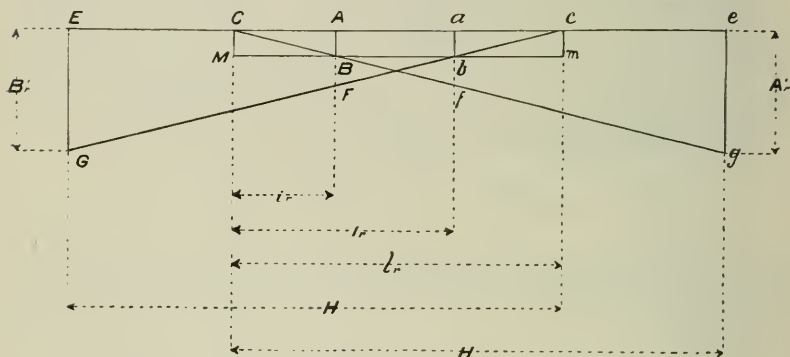


FIG. 2.

In this figure  $\overline{Cc}$  is the span and  $\overline{CA} = i_r$ ,  $\overline{Ca} = I_r$ ,  $\overline{EG} = B'_r$ ,  $\overline{eg} = A'_r$ . The latter two are laid off to a large scale. Also  $\overline{Ec} = \overline{Ce} =$  is the length of a line that an arbitrary force  $H$  (usually the pole distance of the force polygon) would be, if constructed to the same scale that  $P$  (the load) is taken, in the graphical determination of  $A'_r$  and  $B'_r$ . It is better to have this scale considerably greater than is convenient for the force polygon or shear line. Since  $A'_r$ ,  $B'_r$  and  $H$  are laid off to the same scale, the tangents

$$\frac{A'_r}{H} \text{ and } \frac{B'_r}{H}$$

remain the same for any scale.

From the drawing we obtain the following :

$$\overline{ab} = \frac{B'_r}{H} (l_r - I_r)$$

$$\overline{af} = \frac{A'_r}{H} I_r$$

and subtracting

$$\overline{bf} = \frac{A_r'}{H} I_r - \frac{B_r'}{H} l_r - I_r$$

and therefore

$$\overline{CM} = \frac{\overline{bf} \cdot \overline{CA}}{\overline{Aa}}$$

$$= \frac{I_r i_r \frac{A_r'}{H} - \frac{B_r'}{H} i_r (l_r - I_r)}{I_r - i_r} = \frac{M_r}{H}$$

$$\overline{AB} = \frac{A_r'}{H} i_r \quad \overline{AF} = \frac{B_r'}{H} (l_r - i_r)$$

$$\overline{BF} = \frac{B_r'}{H} (l_r - i_r) - \frac{A_r'}{H} i_r$$

whence

$$\overline{cm} = \frac{\overline{BF}}{\overline{Aa}} \cdot \overline{ca}$$

$$= \frac{\frac{B_r'}{H} (l_r - i_r) (l_r - I_r) - \frac{A_r'}{H} i_r (l_r - I_r)}{I_r - i_r} = \frac{M_{r+1}}{H}$$

The diagram required for the moments due to the sinking of the  $r^{\text{th}}$  support, a distance  $h$  below the level of the rest is the same as that given in this *Journal* for November, 1889, except that for variable moment of inertia we are to take (in that diagram)

$$\overline{Dp} = \frac{h}{L_r l_r H}$$

which for constant moment of inertia reduces to

$$\frac{\theta h}{H l_r^2}$$

which is equivalent to the expression given in that article.

This diagram can be used for the sinking of every support, except the first and last. For these, formulæ which themselves indicate the simple diagrams required, would be convenient.

For the first support we want  $M_2$  and  $Y_2$  is the only  $Y$  that is not  $= 0$  in the original equations, and this has the value

$$\frac{h}{l_1}$$

and we, therefore, have

$$M_2 = \frac{d_2 c_2 Y_2}{Z_s} = \frac{d_2}{Z_s} \frac{h}{l_1}$$

Make  $r = 1$  in equation (11a), and we have

$$Z_s = (c_1 d_2 - c_2 d_1) L_1 = -d_1 L_1$$

whence

$$M_2 = -\frac{h}{L_1 l_1} \frac{d_2}{d_1} = \frac{h}{L_1 l_1} \frac{(l_1 - I_1)}{I_1}$$

In the same manner for the last or  $(s+1)^{\text{th}}$  support we want  $M_s$  and  $Y_s$ , is the only  $Y$  that is not  $= 0$  in the original equations, and it has the value

$$\frac{h}{l_s}$$

and we, therefore, have

$$M_s = \frac{c_s d_s Y_s}{Z_s} = \frac{c_s}{Z_s} \frac{h}{l_s}$$

Make  $r = s$  in equation (11a) and we have

$$Z_s = (c_s d_{s+1} - c_{s+1} d_s) L_s = -c_{s+1} L_s$$

whence

$$M_s = -\frac{c_s}{c_{s+1}} \frac{h}{L_s l_s} = \frac{h}{L_s l_s} \frac{i_s}{l_s - i_s}$$

The quantities

$$\frac{A_r}{L_r l_r} \text{ and } \frac{B_r}{L_r l_r}$$

for constant moment of inertia reduce to  $P(2k - 3k^2 + k^3)$  and  $P(k - k^3)$ , respectively, where  $P$  is the load and  $k$  is the ratio of the distance of the load from the left support to the length of span. The graphical method of obtaining these quantities was given in this *Journal* for November, 1889.

The foregoing method of obtaining unknown quantities by fixed points may be called grapho-algebraic. That is, it may be applied to any problem in which the equations have this form. The modifications of formulæ necessary for coefficients different in each equation will suggest themselves after a little study, but are not developed here for want of practical utility. The fixed points would be the same, but the expressions for them would be different. The diagram for obtaining moments would be unchanged if the conditions by which the  $Y^s$  co-exist were the same as for the continuous girder.

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## THE ALUMINIUM PROBLEM.

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[*A lecture delivered before the Franklin Institute, January 12, 1891.*]

The lecturer was introduced by Prof. Persifor Frazer, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

Before beginning my lecture proper, I should say that shall take a liberty which a very few years ago would have been a presumption, but which to-day, and especially in this place, will, I do not doubt, be readily granted me. I refer to the fact that I am going to assume on the part of my audience some, perhaps considerable, familiarity with the general properties of this new metal. Within a year or so, two well-known Philadelphians have discoursed in this hall about aluminium, its appearance, remarkable properties, applications, etc., and I intend to repeat to-night very little of what has been said here before.

The title of my lecture is "The Aluminium Problem," and for the time being, taking these words as my text, we will first direct our attention to the word "aluminium," the metal of which our discourse treats. It may be interesting

to begin by inquiring how this word should be spelled and pronounced. You all know that two ways of spelling it are in common use, both Webster and Worcester sanctioning either way, Webster giving *aluminum* as preferable, and Worcester *aluminium*; you have also observed that I use the longer spelling. Let us inquire into the history of this matter.

Sir Humphry Davy was the first to give a name to the metallic basis of alumina, although it is not probable that he ever isolated the metal. He said, in 1808, that if he had been successful in decomposing alumina and isolating the metal in it, he would have suggested for it the name *aluminum*. Davy evidently intended this word to represent "the metal from alum," simply taking *alum* and adding *ium*, as the proper termination for a metal. Objections were at once made to this name, not to the termination *ium*, which was considered absolutely proper, but to the root or stem of the word. It was maintained both in France and Germany that the name of the new metal should be derived from its oxide, and not from such a complex salt as alum, of which it happened to be an ingredient. The French name for the oxide was *alumine*, so they formed and have always used *aluminium*. The German name for clay was *Thon*, and for alumina *Thonerde*, so Gilbert suggested, in his famous *Annalen*, to call the metal *Thonerde-metall*, or, for short, *Thon-metall*. Fortunately, none of the German writers adopted this suggestion, but most of them adopted *aluminum*. I find one instance of the metal being called, in Sweden, *argillium*, from the French word for clay, *argille*.

Whether Davy recognized the justice of these criticisms, or whether he considered that *aluminum* needed an extra consonant simply for euphony's sake, we find him, in 1812, making bad worse by spelling the name *aluminium*. It is probable that he felt that the French were correct, but was unwilling to conform entirely to their mode of spelling. However, the name first proposed by Davy had taken root in Germany, and in 1812 we find the English using *aluminum*, the French *aluminium*, and the Germans *aluminium*. The French have always used *aluminium*. The Germans used



*aluminum* exclusively until 1824, when Wöhler, translating a paper of Berzelius from the Swedish used *aluminium*, which he also used in his famous memoir describing the isolation of the metal, in 1827, and which has been used in Germany almost exclusively ever since.

As to the English, they used *aluminum* altogether, until Wöhler's discovery, when the spelling favored by Wöhler was almost universally accepted, *aluminium* being used only now and then by some patriotic Englishman, averse to foreign innovations. We may say, therefore, that since 1827 the scientific world accepted and used the term *aluminium*.

Americans, however, have been most active in resurrecting the old English term *aluminum*. They say it is the shorter (true) and *ergo* the better way. That is what I should call intensely American. Mr. Eugene H. Cowles tells us that the Cowles Electric Smelting and Aluminum Company was originally organized as the Cowles Electric Smelting and Alumin(*i*)um Company, but that the writing of that extra *i*, and especially the dotting of it became such an insufferable burden and expense that they went to the court about it, and had the name changed to the Cowles Electric Smelting and *Aluminum* Company.

The question now is, which of these two forms shall we use? If *aluminium* is positively too long for practical, everyday use, and must needs be shortened, it is mere trifling to say that the dropping of one *i*, the removal of one letter out of nine, is any satisfactory means of accomplishing that end. But, if a shortening must be made, if manufacturers need it and public opinion demands it; if, above all, it would, in any way, save dollars and cents and lower the market price of the metal, then let us make a shortening which will be radical, which cannot be improved upon, and adopt Mr. Oberlin Smith's suggestion to call the metal *alium*. I think the suggestion a happy one, and if any change from *aluminium* should become necessary, I should be in favor of, and be one of the first to use, *alium*.

We will next consider what ground is intended to be covered by the word "Problem" in the title of my lecture.

There are many problems connected with aluminium. Primarily, of course, there is the problem of its production; next, that of its applications—casting, drawing, tempering, hardening, welding, polishing, turning, spinning, stamping. One very important question is soldering this metal. Then there is the problem of alloying it correctly and in proper proportions with other metals. The chemical analysis of aluminium is also a subject requiring more study than has yet been given it. Of all the problems connected with this new metal, I have singled out the one which is, par excellence, *the aluminium problem*, and that is, *the extraction of aluminium from the materials in which it is found in nature*. This is the one problem of paramount interest to the metallurgist.

Let me now divide my lecture into two parts, which may not appear at first sight to differ much from each other, but which are, indeed, very distinct. I wish to consider the aluminium problem as I have defined it under the two headings: (1) The isolation of aluminium. (2) The production of aluminium cheaply. The principle on which this division is made is not only logical but chronological; the first part intends to treat of the many efforts which were made to bring aluminium to view, from the earliest recorded experiments on the subject to the time when the first lump or pencil of aluminium was made by Deville, in 1854; the second part will include all subsequent methods.

We will inquire, in the first place, how the existence of the element aluminium became known to science. To do this we will start as far back as the times of the Romans, whose historian Pliny tells us that *alumen* was a name applied to several kinds of salts used in dyeing, a white kind for dyeing bright colors, and a dark for dull colors; that all alumen had an astringent taste and was colored black by the juice of the pomegranate. From all we can learn from Pliny and Columella, it seems that alumen was, therefore, a mixture of aluminium sulphate with more or less, always with some, sulphate of iron. Why it was called *alumen* is not certainly known. About the eighth century, we hear of a very pure *alumen* being made in Egypt and

afterwards in Rocca, near Smyrna, Asia, and hence called *alumen Roccæ* or *rock alum*. This was, indeed, a pure sulphate of alumina with sulphate of potash; in fact, our modern alum, differing, indeed, from the old *alumen*, but since it was used for the same purposes it was given the name of *alumen* or *alum*, and considered simply as being a very pure kind. This explains how our modern alum received its name.

The vitriols, sulphates of iron and copper, were also included under the term *alumen* or *alum* until about the twelfth century. Even in the thirteenth century, alum and vitriol were named together as related substances. The alchemist, Paracelsus, first strictly separated these salts, on the ground that the base of the vitriols was metallic and that of alum earthy. We understand, then, that in the beginning of the seventeenth century, alum was considered to consist of sulphuric acid and an earth whose nature was unknown.

Ettmüller, in 1684, in his *Chymia Rationalis ac Experimentalis*, says: "Alum is obtained by acting on clay with sulphuric acid." He had evidently discovered that the earthy base of alum is contained also in clay.

Stahl, in 1702, propagated the idea that this unknown earth was of the nature of lime or chalk, and for over fifty years most chemists followed this view.

In 1722, Hoffman announced that the earth of alum was of an entirely distinct nature from any other earth then known. Geoffroy in 1728, Hellot in 1739, and Pott, in his famous *Lithogecognosia*, in 1746, all reiterated this statement with increasing certainty until it was finally received as an accepted fact, and the last-named chemist gave it the name of *Thonichte Erde* or *terre argilleuse*.

In 1754, Marggraff, in three able dissertations on alum and its earth, showed that this earth was certainly a distinct substance; that it existed in all natural clays, can be extracted therefrom by sulphuric acid; that the part of the clay not touched by the acid is silica, and that, therefore, the purest white clay contains only silica and the earthy base of alum. These memoirs settled conclusively the

composition of clay, and gave rise to the term *argil* or *argil pur* as the name of the earthy base of alum. It was thus called until 1762, when Morveau, intent on revising chemical nomenclature, decided that since alum was called *sel alumineux*, the proper name for its earthy base should be *alumine*, thus avoiding all confusion with *argille* and *terre argilleux*. The Germans, however, still call 'the earth of alum *Thonerde*, the French call it *alumine*, which the English also did until about 1820, when it was anglicized into *alumina*.

I left off with Marggraff in 1754, by whom the question of the composition of clay and the individuality of the base of alum was finally settled. Soon after this are to be found the earliest indications that alumina was suspected to be a compound body, and the search after aluminium began.

In 1758, Macquer wrote as follows: "The earth of alum is white, infusible, and since it changes color on being heated, I suspect it has some sort of a relation with the metallic earths." He was arguing from wrong premises but his conclusion was, by accident, not far wrong.

And now we come to the first recorded attempts to isolate aluminium. The experimenter's name was M. Baron, a professor of chemistry in Paris, who, in 1760, communicated a memoir to the Académie Royale, in which he says: "I believe the base of alum to be of a metallic nature for the following reasons: It has almost no properties in common with the known earths, while it has analogies with the metallic earths, such as changing color when heated, its astringent salts," etc. Then M. Baron tells us that he tried all known methods of reduction, but could not succeed in getting the metal. He concludes by saying: "If I had been fortunate enough to reduce the base of alum to a metal, no other argument would be needed, and that which to-day I conjecture would be a demonstrated fact. I am far from regarding the problem as an impossibility. I think it not too venturesome to predict that a day will come when the metallic nature of the base of alum will be incontestably proved." It is to be regretted that M. Baron does not record the manner in which he made his experiments, but



it is probable that they were mostly concerned with attempts to reduce alumina by mixing it with carbon or some organic substance, with some such additions as salt or black-flux, and heating as highly as possible in a coal or charcoal fire. Nevertheless, it is some satisfaction to know that he tried all the methods of reduction then known.

In 1782, Lavoisier, investigating the principles of combustion and oxidation, arrived at the conclusion that alumina was the oxide of a metal, the affinity of the metal for oxygen being so strong that carbon was not able to overcome it, and the oxide was irreducible by any hitherto-known process. Lavoisier, moreover, succeeded in fusing alumina in a charcoal fire fed by pure oxygen.

In 1790, two Austrian chemists, Messrs. Ruprecht and Tondi, made experiments on reducing alumina. They thought that hitherto the reducing agent had not been intimately enough mixed with the alumina, and that the temperature used had not been sufficiently high, so they mixed alumina very intimately with charcoal dust, made into a paste with oil, and spread it on the inner walls of a Hessian crucible, the crucible being finally filled up with powdered charcoal, a layer of bone ash being placed on top. Putting the crucible in a forge, a strong fire was kept up for three hours. As a result, small metallic particles were found on the sides of the crucible. These they supposed to be the metallic base of alumina.

M. Savaresi in Italy, and Klaproth and Karsten in Germany, repeated this experiment and analyzed the globules of metal obtained. They found them to be phosphide of iron, the iron coming from the charcoal used, the phosphorus from the bone ash. Prof. Klaproth referred to this incident as "the pretended metallization of the earths," and said further, "If there exists an earth which has been put in conditions where its metallic nature should be disclosed, if it had such, an earth exposed to experiments suitable for reducing it, tested in the hottest fires by all sorts of methods, on a large as well as a small scale, that earth is certainly alumina, yet no one has yet perceived its metallization."



In 1807, Sir Humphry Davy, fresh from his victory over potassium and sodium by the aid of his great battery, attacked the alkaline earths in a similar manner. His first experiment, mixing alumina with red oxide of mercury, and passing the electric current through this in contact with mercury, was a total failure. Davy then began to think in earnest how he might reduce alumina. He remembered that potassa has a strong affinity for alumina, uniting with it easily, and that since the oxygen in each of these compounds could not be the cause of their affinity for each other, it must be attraction between the two metals. He then argued that if he first combined the two oxides and then put the compound under conditions in which potassium, if alone, would certainly come down, that in so doing it might possibly, because of its affinity for the metallic base of alumina, induce it to come down also. This was an idea worth trying, so he melted together one part of alumina with six parts of potash, in a platinum crucible, then made the crucible the positive pole of his battery and a platinum blade dipping into the fluid the negative. On passing the current from his battery of 500 plates, the result partially justified his expectations, for the blade soon received a metallic coating, and on removing it and dipping it into water the water was violently decomposed, the solution being found to contain alumina. An experiment with soda instead of potash gave a similar result. But the metal of alumina was always deposited with the alkaline metal, and Davy was unable to separate it by itself. In fact, when electrolyzing a compound of alumina with caustic alkali, both metals must necessarily be set free if the compound is decomposed at all.

Davy's next idea was that it might be possible to reduce alumina by potassium itself, so he mixed some potassium with alumina, heated in a glass tube and observed that some potassium oxide was formed. This showed that some reduction had taken place, but he could see no metal of the earth. In the light of Winkler's recent experiments with alumina and magnesium vapor, we can probably conclude that some aluminium protoxide was formed.

A little later, in 1809, Davy had a new battery of 1,000 plates constructed, and experimented with this on alumina. He fused iron to whiteness in the arc, in contact with alumina, which became quite fluid at the temperature obtained. The iron became somewhat whiter, and when dissolved in acids showed that it contained aluminium. The fact was thus established, that *alumina can be decomposed, while fluid, in the electric arc, and its metal alloyed with iron.*

Davy was spurred to renewed endeavors. He placed alumina in a platinum tube and heated it to whiteness, meanwhile passing vapor of potassium over it—a most difficult experiment to perform. After cooling, mercury was introduced, in order to collect the metal if any had been isolated.

The mercury showed on solution only a trace of alumina, so that Davy was not sure if any alumina had really been reduced. The experimenter next mixed alumina with potassium and iron filings, hoping that if the potassium did reduce any alumina the iron might alloy with the metal. This experiment gave the best results obtained by Davy. On fusing the mixture a button resulted which was white and harder than iron, and undoubtedly consisted of an alloy of iron and aluminium.

Davy proved, then, that *fused alumina can be decomposed in the electric arc*, and that *alumina can be decomposed by potassium in the presence of iron*, but he was unable in any way to isolate the metal he sought for.

Prof. Benjamin Silliman went perhaps a step farther than Davy. Dr. Hare, of Philadelphia, had invented the oxy-hydrogen blow-pipe in 1802, and with it fused alumina to a milk-white enamel. In 1813, Prof. Silliman was repeating this experiment, supporting the alumina on charcoal, when he noticed small metallic globules rolling and darting out from under the fused mass and burning with a bright light. These globules could have been nothing else but *aluminium, reduced from fluid alumina by charcoal.* Thus another step was gained in the solution of the aluminium problem. The presence of oxygen at such a high temperature immediately oxidized these globules, and Prof. Silliman was unable to obtain the metal.

We now come to another advance, which was destined to lead ultimately to the solution of the aluminium problem as far as isolating the metal was concerned. The problem was attacked by a flank movement. Alumina could not be satisfactorily decomposed, but Oersted, in 1824, discovered a way to make a new salt of aluminium, its anhydrous chloride,  $\text{Al}_2\text{Cl}_6$ , and this opened the way to Wöhler's success three years later. Previously to 1824, aluminium chloride was known only in solution, as when alumina was dissolved in muriatic acid; but, on evaporating this solution, hydrochloric acid fumes pass off and alumina remains. Oersted passed chlorine gas over red-hot alumina, mixed with charcoal, contained in a porcelain tube. Anhydrous aluminium chloride distilled over into a condenser. It then occurred to him that perhaps potassium would reduce this new salt. The idea was right, but he made the mistake of using potassium amalgam instead of pure potassium, thinking that he might thus get an amalgam with aluminium and distil the mercury away from it. Potassium when amalgamated with mercury, however, is not powerful enough to reduce aluminium chloride, and Oersted missed, by a very little, the honor of first isolating aluminium.

Berzelius, the Swedish chemist, was a constant worker at the aluminium problem. He wrote to Sir Humphry Davy, in 1809, that he had reduced alumina by heating it with carbon in the presence of iron, but it is probable that he was mistaken, because he never refers to this in any of his later writings. However, after many attempts and repeated failures, he once came within an ace of solving the problem. He had studied carefully the mineral cryolite ( $\text{Al}_2\text{F}_6 \cdot 6\text{NaF}$ ) and determined its composition and chemical formula. He made it artificially, and also the corresponding potassium salt. It then occurred to him that this latter salt might be reduced by metallic potassium. He made the experiment in a crucible, and carefully washed the fusion with water, but found no metal. It is quite evident that his sole mistake was in using an excess of potassium, which, after the reduction, gave on solution in water a caustic alkali solution

which immediately dissolved all the reduced aluminium. If he had used an excess of fluoride salt the story would almost certainly have read differently, because this reduction has since then been repeatedly performed. The date of this experiment was 1825.

In 1827, Wöhler, the German chemist, after repeating Oersted's experiment with unsatisfactory results, ventured to modify the experiment by using pure potassium instead of its amalgam. He placed a lump of potassium in the bottom of a platinum crucible, covered it with an excess of aluminium chloride, and fastened on a cover. He then heated the crucible gently, waited until the reaction was over, and treated the resulting fused mass with water. *He obtained a gray, metallic powder, which was finely-divided aluminium.* This was the first real solution of the aluminium problem. Wöhler could not get this powder to gather into a button, in order to determine the properties of the metal *en masse*, but he found out many of its chemical properties, such as its resistance to acids, solubility in alkalis, etc., and produced from the metal some of its compounds, such as the sulphide and arsenide, which had not been known before. His observations were vitiated, however, by the fact that the powder contained platinum, which decreased its fusibility, and some potassium, which rendered it less resistant to oxidation.

At this point an unexpected obstacle arose to further advance. This powder could not be in any way united to a button. I have no doubt that Wöhler and others tried every conceivable way of doing this, but for many years without success. This was, to say the least, vexatious.

The question remained in this state until 1845, when Professor Liebig tried whether the reduction would proceed any easier if vapor of aluminium chloride were passed over potassium. He found this arrangement much preferable to Wöhler's old manner of working, but still nothing was obtained but the gray powder. Wöhler was still able to work at the subject, and he tried passing potassium vapor over aluminium chloride, with the long-sought-for result of *obtaining some globules of aluminium.* These were not much



larger than pin heads, while the powder accompanying them was no longer in scales, but was shown by the microscope to consist of numerous fine globules. This was encouraging, and Wöhler was able to determine that the metal was malleable and to get its specific gravity quite closely. But, just here, he made a fatal error. He picked out the largest globules to try to melt them into one large button, but used for a flux borax, not knowing that it attacks the metal strongly. Besides, the melted borax being of higher density than aluminium, the globules remained separated on its surface, where the oxidizing influence of the air kept them from uniting. The result was that Wöhler saw the globules get smaller and finally disappear, and hence concluded that they had oxidized away, and that aluminium was not a very stable metal after all.

The satisfactory solution of the aluminium problem was thus delayed until 1854, when Henri St. Claire Deville took up the question. He says that he did not know of Wöhler's results of 1845, and was repeating Wöhler's first experiments, always getting the gray powder. On modifying the experiment as Wöhler had done, he obtained globules similar to Wöhler's later results. This success encouraged him to renewed efforts to get the metal in mass. He found that metallic sodium was much safer to work with than potassium, performed the reduction just as well, and, further, enabled him *to collect the metal into a large button*. The reason for this latter result was that when the sodium acted on the aluminium chloride, the sodium chloride formed combined with the excess of aluminium chloride to form a double chloride,  $\text{Al}_2\text{C}_6\text{1.2NaCl}$ , which was very fusible and acted perfectly as a flux to unite the metal to a button, being lower in density than aluminium and apparently dissolving away the coating on the globules which prevented them from running together. Further, since he operated in a glass or earthenware retort, the metal was free from platinum; he also obtained it free from sodium, and almost chemically pure.

We have now followed the aluminium problem through its purely experimental stage. We have passed in review



a history of ninety-four years of recorded experiments, from Baron, in 1760, to Deville, in 1854, and the first part of our problem is solved. Aluminium, *en masse*, can be isolated from its compounds.

By a most natural transition we pass directly to the second part of the aluminium problem, as I stated it in beginning my lecture, namely, "The production of aluminium cheaply." This problem is almost radically different from the first. During all those ninety-four years, expense was never once mentioned as entering in any way into the problem, but now the question is changed. We may conceive the fair goddess who presides over these secrets of nature to have said to Deville, "You have solved my first problem; you can show me a piece of aluminium?" "Yes" "Then I give you a harder one: produce it cheaply."

As Wöhler was the principal factor in solving the first problem, so was Deville the prominent figure in the solution of the second. His task was well-nigh herculean. I suppose that the first half-ounce lump of aluminium which he produced cost \$50. He nobly bent all his energies to the work before him.

Before reviewing the work of Deville and other investigators since 1854, let us look for the real reason for all this serious work. What is the great fact that underlies the search after cheap aluminium; what is the main-spring of the aluminium industry? It is not that aluminium is the peer of all metals, for gold is more beautiful, platinum more stable, iron more useful; but it is the fact discovered by Deville that aluminium is in many respects a beautiful, durable, useful metal, coupled with the other great fact, known for a century, that next to oxygen and silicon it is the most abundant element to be found on the earth.

It is little wonder, then, that not only Deville but also chemists in all countries, of all nationalities, have spent time, money and brains without stint to advance the problem of its extraction. It is also little wonder that so many capitalists interested in the subject have often been induced to put their money into the hands of charlatans

and frauds, for the ostensible purpose of working at the aluminium question. It is not at all to be wondered at that this question has become one of the most noted metallurgical questions of our day, and its satisfactory solution will, I am sure, crown the last decade of this wonderful century.

The problem must now be considered on a commercial as well as a metallurgical basis. The question is, "What is the cheapest raw material to use, and what is the cheapest way of extracting the aluminium?"

Of all the natural aluminium compounds, clay is probably the most widely distributed. White china clay, or kaolin, is the purest, containing about equal parts of alumina and silica, or about twenty-five per cent. of aluminium. Colored clays contain less alumina and varying quantities of such impurities as iron oxide, lime, etc. Kaolin is abundant and cheap enough, and many of my audience have perhaps wondered why aluminium is not at present extracted from it. The reason is that by any of the methods used for producing aluminium, any silica present with the aluminium compound being reduced is invariably reduced to silicon, which alloys with the aluminium and injures very much its commercial value, a large percentage making it entirely unfit for use. It may then be asked, "Cannot the clay be first treated in such a way as to remove the silica?" True, it can, but to do this costs money, and the alumina thus obtained would be more expensive than that produced from other natural minerals. (I hope that this explanation will be satisfactory to those of my audience who have been in the habit of thinking of aluminium as "the metal made from clay." About a week ago I met a gentleman in the south, a colonel, of course, who, when I replied that aluminium was not being made from clay, remarked, "What, not being made from clay! Why, that takes all the poetry out of it," and his interest in the subject was so far gone that he changed the conversation.)

The two minerals which serve the aluminium industry for raw materials are cryolite and beauxite. Cryolite is a double fluoride of aluminium and sodium,  $\text{Al}_2\text{F}_6 \cdot 6\text{NaF}$ , containing nearly thirteen per cent. of aluminium, while beauxite is an impure hydrated alumina containing from twenty-five to

thirty-five per cent. of aluminium. Cryolite is only found in quantity in Greenland, the mines are monopolized, and although costing only about \$20 a ton to land in this country, the importers charge purchasers several times that price for it. Cryolite also contains some silica finely disseminated through it, which much impairs its usefulness for producing pure aluminium. Beauxite is really the most suitable mineral for furnishing alumina. It occurs abundantly in deposits in the south of France, Austria, Ireland and in Georgia. Its cost is from \$6 to \$10 a ton. Average specimens of the Georgia mineral contain one to two per cent. of silica and one and one-half to three per cent. of ferric oxide. With such a mineral to be bought at the price named, it is the cheapest source of alumina that the aluminium industry can find.

Returning to Deville, we will follow him in his task. About the first improvement he made was to reduce the cost of metallic sodium. He erected apparatus on a large scale, and reduced its cost from \$100 a pound in 1855 to \$1 a pound in 1859. Since it takes about three pounds of sodium to produce one of aluminium, the saving thus effected was a large proportion of the cost of the aluminium. His next step was to replace aluminium chloride by the aluminium-sodium double chloride. He found that this latter salt was less apt to deteriorate in the air, less volatile, more fusible and altogether much more satisfactory to work with, while it could be produced with even less trouble than aluminium chloride. A further improvement was to operate the reduction on a large scale, on the bed of a reverberatory furnace. Putting all these economies together, Deville reduced the selling price of aluminium in 1862 to \$12 a pound, at which figure it stayed until 1887; that is, for twenty-five years. This shows the net result of Deville's labors, but it gives no idea of the hundreds of experiments which he made during those years. He tried electrical methods (of which I shall speak presently), the use of cryolite; he tried to produce sodium vapor from one retort and aluminium chloride vapor from another, and mix them in a third retort or reduction chamber. In fact, although his

works earned considerable money, yet he spent it almost all in experiments, and in 1859 he crowned his work by writing his book, *De l'Aluminium*, which is one of the most admirable treatises known to science.

Devil's labors and the process which he developed have been very ably supplemented by the work of Mr. Castner, who, by a very ingenious invention, reduced the cost of sodium to about twenty-five cents a pound, and by establishing the manufacture of aluminium-sodium double chloride on a large scale, lowered its cost about three-fourths. The result was that the works managed by Mr. Castner sold aluminium in 1888 at twenty shillings a pound, and later as low as ten shillings, but their process is now equalled in cheapness by the recent electric processes, and the fact is now apparent that the sodium processes for producing aluminium are being worsted by electricity, and their description will soon become a matter of history.

A few words only are necessary to describe the reduction of cryolite by sodium. You remember that Berzelius tried, in 1824, to reduce it by potassium, and was unsuccessful, because he used an excess of the reducing agent and washed the fused mass with water. When Deville made known his method of working on aluminium chloride, in 1855, Dr. Percy, in England, tried to produce aluminium by reducing cryolite in a somewhat similar manner, and was successful in getting the metal. H. Rose, of Berlin, also made similar experiments. The reduction was effected in iron crucibles, because cryolite corroded common crucibles, and the silica dissolved contaminated the metal with silicon. On the other hand, Rose's metal was often ferruginous. Deville gave this subject considerable attention, but decided that the return of metal was small, the metal obtained was impure, and, above all, the supply of cryolite was precarious and would not serve as the basis of a large industry. However, the Tissier Brothers built a works near Rouen, France, in 1857, based on the use of cryolite and its reduction by sodium. Their best metal was always siliceous, and they were in operation only about five years. Of late years, the cryolite method has been revived by the Alliance Alumin-



ium Company, which owns Dr. Netto's process for manufacturing sodium, by which this metal can be made nearly as cheaply as by Mr. Castner's process. In fact, they are selling sodium cheaper than the Aluminium Company, Limited, owning Castner's patents. The method adopted by this company was to melt a large quantity of cryolite, pour it into a ladle, and by means of an inverted cup force to the bottom of the ladle a large lump of sodium. The operation depended for its success on the large scale on which it was operated. This company competed for some time with the other sodium processes, but it is now eclipsed by the electrolytic methods.

We now come to the consideration of a part of my subject, which is essentially very interesting—the application of electricity to solving the aluminium problem. We have noticed its application by Davy, in 1807 and 1810, with only partial success. We will take it for granted that probably before Davy's time, and certainly between 1810 and 1855, the galvanic deposition of aluminium from aqueous solutions of its salts had been often attempted and without success. I may say here that since this mode of electro-deposition is, even when successfully operated, only suitable for producing thin coatings of aluminium, and not to producing aluminium *en masse*, I shall leave it out of further consideration this evening.

Somewhere about 1850, Prof. Bunsen made a new departure in electrolytic science by using as an electrolyte a fused bath of magnesium chloride and thus produced magnesium electrolytically. When Deville set about the aluminium problem in earnest, in 1855, he thought at first that the sodium process was not capable of much cheapening, and he cast about for some other method of production. Knowing of Bunsen's results with magnesium, he thought it possible that a fused bath of aluminium-sodium double chloride might answer as an electrolyte, aluminium chloride alone not being suitable because its melting point and volatilizing point are so close together. He tried the experiment, found that it would succeed, and in this way produced more aluminium than up to that time he had



made by the sodium process. The operation, however, was beset with difficulties. Carbon electrodes were used, and these disintegrated so quickly as to interfere seriously with the electrolysis. Bunsen was working at the same time in a similar manner, and the two investigators published their results almost simultaneously.

These experiments were really successful, but they were not brought into practical use at that time because the only source of the electric current at their disposal was the galvanic battery, whose costliness put a stop to all hopes of commercial usefulness. As soon, however, as magneto- and dynamo-electric machines placed at command large electric currents at small cost, these old experiments were quickly put into practice. Berthaut, in 1879, proposed the use of dynamo-electric machines in producing aluminium, and Grätzel, in 1883, made their first commercial application. Since then, numerous processes of this kind have been proposed and put in operation, differing from each other principally in the composition of the electrolyte used and the arrangement of the electrodes.

The conditions to be fulfilled in electrolyzing fused aluminium compounds are that the salt shall conduct electricity and that the current be of sufficient tension to decompose it; and then, in order that the aluminium liberated be practically obtained, there must be no opportunity given for a re-combination of the separated elements, and the bath must have no solvent action on the liberated aluminium. We will see how these details have been arranged in the different electrolytic processes.

Gaudin, in 1869, proposed the use of an electrolyte composed of equal parts of cryolite and sodium chloride. The advantage of this mixture was that it melted at a low temperature and was very fluid, its disadvantage was that at the high potential required to decompose the aluminium fluoride, the sodium chloride present was also decomposed and much sodium vapor liberated.

Kagensbusch, in 1872, proposed to melt clay with sodium chloride, and electrolyze, a metal being present to alloy with the aluminium as it was liberated. The trouble here is, of

course, the silicon, which is reduced along with the aluminium and renders the product worthless.

Grätzel used a dynamo-electric current, the electrolyte being cryolite or aluminium-sodium chloride, while the pot containing the electrolyte was of aluminium and made the negative electrode, and the positive electrode was a mixture of alumina and carbon pressed into shape. The particular advantages of this process were that the metal collected on the walls of the pot, while the chlorine or fluorine liberated at the positive pole was in immediate contact with alumina, on which it was supposed to react and thus regenerate the bath. A great deal of aluminium was made by the company working this process, but they state that the Grätzel process, as originally patented, was a failure, principally because of the disintegration of the positive electrode and the consequent clogging of the bath. The improvements which they have made to this process have not been made public.

Dr. Kleiner, of Zurich, used as an electrolyte pure cryolite, the electrodes being two pencils of gas-retort carbon, but he overcame some of the difficulties of the electrolysis in a very ingenious manner. The chief difficulty in working with cryolite is to get material for the vessel which contains it which will not be corroded by the molten salt. Dr. Kleiner filled a crucible with cryolite and imbedded in the middle his two electrodes. The current being turned on, these carbons were drawn far enough apart to form an electric arc, which very quickly melted the cryolite for a space of several inches around it. Part of the energy of the current was thus converted into heat to keep the electrolyte fluid, while the current also operated its decomposition. The process was not successfully operated, the chief difficulties being the corrosion and disintegration of the carbons and the accumulation of sodium fluoride in the bath, increasing very much its electrical resistance. However, the device of keeping the electrolyte fluid by part of the energy of the electric current has many advantages, and although it is rather a costly method of obtaining heat where steam-power is used for the dynamos, yet it is not

very costly where water-power is used, and it is an improvement which is being used in all the present successful electrolytic processes.

Hèroult, Hall and Bernard Brothers have patents covering methods of working somewhat similar to each other. They all electrolyze cryolite or an electrolyte of analogous composition, the bath being kept continually saturated with alumina. The effect of passing the current is in the end to separate aluminium at the negative pole and oxygen at the positive, and the principal difference between these processes is in the different explanations given as to the reactions taking place in the bath. I am informed that the Cowles Electric Smelting and Aluminum Company have also purchased an English patent (Bognski's process) which covers the same principle, and intend working for pure aluminium. The different explanations referred to are as follows: Mr. Hall claims that he decomposes alumina mechanically dissolved in the fused solvent, while other parties claim that the aluminium fluoride is the compound decomposed, but that the fluorine set free immediately acts on the alumina present to regenerate aluminium fluoride and liberate oxygen. Which of these two explanations is correct is not certain. I believe our respected secretary, Dr. Wahl, is on record as thinking the latter explanation more probably correct, while, from a recent inspection of one of the processes, I have been inclined towards the opposite conclusion; so, while the *doctors* differ, the *patient* audience may be left to take its choice.

If asked my opinion of the economy of these processes, I should reply that, metallurgically, they can be brought very near to perfection. Since the solvent remains unaltered by the current, aluminium is practically obtained direct from alumina, the cheapest source of aluminium, in one operation, while, since there is no avenue of escape for anything, no slag, no waste products, practically all the aluminium in the alumina is obtained. The two chief items of expense peculiar to the nature of these processes are, then, the cost of power and plant. With cheap water-power, which can be obtained in many places, the item of power will be reduced very low

—so low that it would no more than balance the expenditure for fuel by any other possible means of reduction. The other item of expense, however, is a standing one. Electrolysis is, primarily, a slow process; immense currents are needed for only moderate outputs; expensive turbines and still more expensive dynamos must be purchased and kept in repair, while, finally, the number of pots needed for only moderate outputs renders the plant expensive, and interest on this capital will always form a large item of the expense of these processes.

There are two electrical processes which I have purposely not mentioned among the electrolytic processes, and I now discuss them here together: Cowles Brothers' and Hèroult's processes for producing aluminium alloys. The principle involved is the interruption of a powerful electric current, the formation of an immense arc, and the reduction in this arc of alumina, by carbon, in the presence of a metal. The working of both of these processes is too well known to require my tarrying to describe them to-night. In Cowles Brothers' furnace the current is interrupted by having to pass through a box filled with broken pieces of carbon, the whole interior is raised to a very high temperature, at which the alumina present is reduced by the carbon and alloys with the metal. That this is the rationale of the process there can be almost no question. The Hèroult apparatus is arranged differently. Alumina is fused by the electric arc, and floating on molten copper, is then treated as though it were an electrolyte, a carbon rod dipping in the alumina being the positive pole and the copper the negative. The patentee of this process claims that it is essentially electrolytic, and indeed it may be allowed that some electrolytic action may take place, because the parts are arranged in just the correct way to obtain that result, but, by a quantitative study of the quantity of the electric current and the amount of metal reduced, both in this furnace and in the Cowles process, it can be shown that several times as much aluminium is produced as the current could be able to separate electrolytically. It is therefore evident that the larger part of the reduction in these furnaces is due to some other agency than electrolytic



action, and this other agency can only be the reducing action of carbon. Of the two furnaces, I think that the Hèroult is the better arranged and in best shape for practical working.

From the preceding observations, we may then consider the fact established that *alumina can be reduced by carbon*, at a temperature somewhere not far above the fusing point of alumina, which is below  $2,600^{\circ}$  C. This opens up the possibility of simpler processes of reduction than any yet known. Is it because the alumina is fluid that carbon reduces it, or is it solely because of the high temperature? I cannot but think that the fluidity of the alumina plays an important part in this reduction. But, may it not be possible to render alumina fluid at a lower temperature than now appears necessary? I think that it may. For instance, when I dissolve iodine in alcohol, rubber in carbon bisulphide, do not these substances become fluid at ordinary temperatures? Cryolite and some other salts appear to dissolve alumina mechanically at about a red heat, and if this action be purely mechanical, then we certainly have alumina fluid at that heat. I think it quite probable that alumina thus rendered fluid could be reduced by carbon at a much lower temperature than is required to do it in the electric furnaces.

You will have gathered from these remarks that I am by no means wedded to the idea that we will find the final solution of the aluminium problem in electrical processes. I consider that they are handicapped by an expensive plant, and although electrical processes are "on top" at present, and I think will stay there for some time to come, yet I am far from considering that they may not possibly be superseded by non-electric methods of reduction. I do not consider the sodium processes, however, as ever likely to take part again in the metallurgy of aluminium. I think their day is past, but I now refer to methods of reduction by common reducing agents. Those who have read the literature relating to this subject must have concluded that almost every possible or impossible means of reduction has been tried, with but slender success. Calvert & Johnson produced iron-aluminium alloys with as much as twenty-



four per cent. of aluminium by the reducing action of iron on aluminium chloride. A similar alloy with three and one-half per cent. of aluminium was made by Faraday & Stodart, by the reducing action of highly-carburetted iron on alumina. Pig irons have been analyzed containing as much as one per cent. of aluminium. Zinc will under some circumstances reduce small amounts of aluminium chloride and cryolite. Half a dozen of the common metals can reduce aluminium sulphide, among which tin seems to do it easiest and most completely. Under peculiar conditions zinc will overcome the affinity of aluminium for oxygen. We have in several places come across the fact that carbon reduces alumina at a temperature elevated enough to fuse the alumina. So, while ordinary reducing agents have been tried in almost every conceivable way, I hope and believe that they will yet come to the front as important factors in producing cheap aluminium.

Gathering all the threads together, catching the drift of all that has been touched upon in this rather rambling discourse, the general conclusion is, I think, that progress, very satisfactory progress, has been and is being made. Rome was not built in a day, and I, for one, am quite satisfied with the progress of the aluminium industry. Six years ago, aluminium sold for \$12 a pound, three years ago for \$5, to-day it is being sold in England at \$1.50, and before this year is out it will probably be down to \$1. Aluminium was never before sold as cheaply as it is now. The prospects for cheaper aluminium were never more promising than they are to-day.

## ELECTRICITY AS THE RIVAL OF STEAM.

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BY DR. LOUIS BELL.

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*[A lecture delivered before the Franklin Institute, January 9, 1891.]*

The lecturer was introduced by Prof. Edwin J. Houston, and spoke as follows:

MEMBERS OF THE INSTITUTE, LADIES AND GENTLEMEN:

It is not my purpose here, to discuss in learned phrase electrical science and electrical transmission of energy, nor to lead you into complicated studies of the practical details of the ways in which we employ electrical energy in daily life. I desire simply to put before you a common-sense view of some of the possibilities and limitations that present themselves when we try to make use of the potent force that is to-day becoming so important a factor in our modern civilization. But before discussing the properties of electrical energy, either for their own sake or for comparison with other things, let us delay for a little and attempt to establish in our minds some sort of definite idea as to what manner of thing electrical energy is, what its most prominent properties are, and how it is connected with the forces that are more familiar to us. We are to-day in a transition stage as regards our view of electrical action. Not so very many years ago scientific men were content to think of what they called electricity as a subtle fluid, to use the time-worn phrase, plastered, as it were, upon material things by certain processes, but with the extraordinary peculiarity that in applying one plaster another similar in some respects but very dissimilar in others, invariably appeared on objects in the vicinity. In other words, electricity was looked on as a fluid that in some way was a mysterious attribute of the surfaces of bodies and produced remarkable effects on neighboring bodies; wherever a charge of the so-called positive fluid appeared another closely similar equal charge of hypothetical negative fluid must appear on bodies

near by. The electric current was looked upon as a flow of this extraordinary fluid through and along a wire much as water flows through a tube; and magnetism by a barbarous sort of analogy was imagined to be composed of two other extraordinary fluids plastered upon the surface of iron and steel bars, and exerting marvellous pulls and pushes on other magnetic porous plasters in the neighborhood. I am going to ask you to-night to turn away from all this and look at the same fundamental facts from a totally different direction.

Now, in the first place, the thing with which we really have to do in every case of electrical action is not the indefinite something called electricity, but a very definite something—electrical energy. We have energy turning up in a great many different forms all about us, as heat, the energy of a falling body, the energy of something thrown, and in almost innumerable other ways. In the case of the energy of a cannon ball, for example, we find it can be measured by the product of the mass of the ball into half the square of its velocity. In looking at almost any other manifestation of energy we find that we can conveniently divide it into two factors, such as mass, velocity; force, and the distance through which it acts; and so on. Now, electrical energy into which we can at will transform the mechanical energy supplied by a steam engine is made up in a precisely similar way of two factors; one of these is potential, the other electricity. For most purposes we may think of electricity, whatever it is, as being only a coefficient of the potential, by which the latter is connected with electrical energy. What the physical meaning of this coefficient may be, in other words what electricity is, we are not prepared to state. It certainly, however, is not electrical energy itself, but only a part of it. Now, experimentally, in every case we are dealing with electrical energy, for we can hardly imagine such a thing as an electrical charge without a potential. If it really exists we have a very small chance of ever becoming acquainted with it, since we can only experiment by means of apparatus that requires a certain amount of work to be done upon it in order to show

any indications whatever. Suppose we produce electrification upon any body by any means we choose, we shall find that the electrified mass will produce a series of pushes and pulls upon light bodies in its vicinity and all about it; in fact, throughout all the region without the body in question we find that there are a series of stresses, minute pulls and pushes, tending to act upon other bodies, and that the whole region is in a state of strain that can readily be made to show itself; a certain amount of energy has been taken from something to set up this effect, which really is potential electrical energy. Now, if we hunt around through the strained region about our electrified body we shall find that every piece of matter in the vicinity has been affected by the stresses that exist, and that its surface is, like the surface of the original body, electrified. Just what goes on at the surfaces themselves is of comparatively little moment, the interesting fact being that the whole region between them is in a state of strain from the electrical energy that has been furnished to it. We should, therefore, think of an electrified body not as having a surface charge of something about which we are totally ignorant, but as the centre of a system of stresses and strains; by stresses I mean forces acting, pushes, or pulls, or twists, or spins; and by strains the displacements, the effect in whatever is subjected to them.

So far as electrification goes, then, it is perhaps sufficient if we think of it as the manifestation of electrical energy that is found at the surface between media having different dynamical properties; just as we find a certain set of phenomena—reflection, refraction, polarization, and the like, at the surfaces between two media that have different dynamical properties regarding light. The familiar fact that electrification is only on the surface of the conductor, follows naturally from this view of the subject, and is only another way of saying that the stresses and strains produced by electrical energy change their character with the substance that is subject to them. Thinking, then, of electrification as merely a surface condition of the electrical energy acting through all the space outside of the surface, we can

easily see the reason for some observed facts in electrical science. What we call lines of electrical force are merely lines which mark the direction of the stresses in the medium surrounding an electrified body. Where these lines of force, that is stresses, act upon matter they produce some sort of strain in it of the kind that we recognize by its effects as electrification. It is quite needless to state that these lines of stress must either terminate in matter, or close upon themselves; for one can hardly imagine such a thing as a strain with only one end, and if we can suppose the sort of strain that is produced by electrification—that is, the communication of electrical energy—to have a certain kind of endedness, as it were, it is not difficult to imagine the reason why the electrification at one end of the line of force is different in quality, but the same in quantity as that at the other end. So we may sum up by thinking of electrification not as the production of a film of electricity on the surface of a body, but as the system of stresses and strains produced around a material body by expending electrical energy upon it. The lines of stress pass outward from the surface of the electrified body to other bodies, in which they produce electrification. In all this there is nothing revolutionary, we are merely looking at the two surface electrifications as terminating a stress in the medium between them.

Now looking at a charge of electricity merely as a centre of stress exhibited in the medium outside, what will happen when that centre moves, as, for example, along a wire? Evidently the character of the stresses in the medium will be somewhat changed, depending upon the exact way in which the movement takes place. Now, as a matter of experiment, we find that when there is such a movement of the centre of electrical stress, as when what we call an electro-motive force is free to act along a conductor, the result is a new set of stresses and strains corresponding to a spin of the medium about the direction of motion as an axis. This spin in the medium is a kind of kinetic energy, the particular kind we call electro-magnetic; but if this view is correct, if we can produce electro-magnetic stresses about a wire by letting a



charge flow along it, it would seem probable that we ought to be able to produce a similar effect by moving the charged body rapidly through space without any conduction whatever. Such is the case, as was shown by some beautiful experiments of Prof. Rowland's about fifteen years ago. Now there is one curious case that can occur with these electro-magnetic spins about the wire; suppose the electromotive force to be an oscillatory one, so that the charge, the centre of stress, will pass rapidly from one end of the wire to the other and back. You can see at once that the result would have to be a pulsating, wave-like strain in the medium, composed still of electro-magnetic spins, but intermittent instead of continuous ones. These pulsations, waves of energy given off from the conductor, may be recognized by their effects as waves of electro-magnetic energy. Like every other sort of strain, push or pull, or whatever it may be, these waves will move out from their starting point with a certain velocity depending upon the medium through which they are passing; in an enormously elastic medium the velocity will be very great, while in a dead, viscous, substance the impulses will be transmitted but slowly. Now, as there will be a certain number of these pulses of energy per second, depending upon the rapidity of the oscillations that caused them, and as they travel at a certain velocity, never mind what it is, each electro-magnetic wave may be said to have a definite length; in other words, each pulse will have travelled a definite distance before the next one starts. Now, it is a very marvellous discovery, suspected twenty years ago, but only proved two or three years ago, that when these waves of electro-magnetic energy are sent out with such rapidity that when the length of each is only about the fifty-thousandth part of an inch, they come within the ken of our senses, and we have all been very familiar with them by their old name of light. If they become a little longer we feel them as radiant heat, and finally we come to such as may be produced by mechanical means, as in an alternating dynamo, where the waves would be something like 2,000 miles long. So we must recognize in light and radiant heat only forms of electro-magnetic energy; or,

rather, we find that they are the same kind of energy that we have been wont to call electro-magnetic.

Suppose, however, that we have no waves, but only steady spins in the medium around the conductor, such as may be set up by the flow of electrostatic stresses, that I have described. The lines of force are then in circles around the wire, and if we coil up such a wire you will see that these circles interlock with the different coils, and we have a set of lines of force shooting through its centre. But such a coil of wire is a magnet, and the addition of an iron core only strengthens, but does not change the character of the stress produced in the medium about it to any appreciable extent. So you see, starting simply with the system of stresses about a material that exhibits the kind of surface strain we associate with electrification, we may come quite easily to the idea of lines of force, having definite ends and producing electrification in other bodies; or, if we imagine the electrification to travel along a wire, it may produce electro-magnetic waves, or steady electro-magnetic force, which can be easily connected to the familiar form of magnetism. Meanwhile, we have scarcely mentioned electricity as such, in fact inasmuch as we cannot experiment on it, since it is not electrical energy, but only a coefficient of it, we can form only very indistinct ideas as to its real character. We know only that what we call an electric charge produces, or accompanies, or *is* a superficial strain in matter, which becomes the centre of a system of stresses in the region outside of it, in which we find much to interest us. The important question is how to produce such a condition of things, for we know by experience that it has very useful properties, as may be seen in the manifold applications of electricity of which I shall presently speak. In theory we can produce mechanical stresses that give an almost identical distribution of energy around the centre of disturbance; for example, if we had a tube open at both ends and immersed in a fluid, and then could suddenly start the fluid in this tube into motion, the lines of flow about the tube would correspond almost exactly with the arrangement of stresses about a bar magnet. A mass of jelly, distorted by a twist applied to a rigid bar imbedded

in it, gives the same distribution of stress that is found in certain cases of magnetism. In similar fashion the electrical stresses may be imitated, so that at all events we need not have to imagine any enormously complicated mechanism for producing the state of things we find. Unfortunately until we know very much more than at present about the intimate properties of matter we hardly know where we should begin work upon it. However, both electrostatic and magnetic action can be very readily produced; in fact, it is very difficult to imagine any operation that can be performed on matter without transmuting energy into one or the other of the forms just mentioned.

In general, however, there are only two convenient ways of getting electrical energy in quantity. By acting chemically on the surfaces of certain metals we can start up, as it were, the kind of stress we call electro-motive force, and get electricity flowing along a wire, but the more convenient method is to start with a magnet; all around it, as we have seen, there are stresses and strains in the medium, and the region where they exist we call a magnetic field. We have said thus far very little about the medium concerned, but we know that it is not air for a vacuum is no obstacle to magnetism, and we know that it permeates all sorts of matter. From the identity of electro-magnetic energy with light we know that the medium in question is what is often spoken of as luminiferous ether. Now, if we push a wire through this region of strained ether about a magnet we find, as might be expected, that the stresses react on the wire, and they actually set up that state of things which we call electro-motive force, hence a current flows in the wire and the energy required to maintain it we have to furnish, and therefore the wire moves a little harder through the magnetic field than outside of it. Now, a dynamo is nothing in the world but a machine for dragging loops of wire through a magnetic field, and thereby setting up currents in the wires. The energy that appears in the current comes from the power that is pulling around the wires. It is just as when one drags a paddle wheel through still water, holding its axle lightly so that it may turn; the wheel can do

work in turning but you have to furnish the power by pulling it along—there is no energy acquired from the water. Conversely, if you pass an electric current through a wire that is in a magnetic field, and thereby set up spins in the medium about the wire these will react on the stresses already in the medium and tend to fling the wire out of the field into a region where no stresses exist; this would correspond, in the hydraulic case we have just mentioned, with a paddle wheel floating in water and turned by hand; it will move along and push things before it, but the power has to be supplied externally. Here we have the principle of the electric motor, which you see is nothing but a dynamo working backwards. In either machine the important things are not the wires, or the iron magnets of the machine, but the actions and reactions set up in the medium that surrounds them. We can readily imagine a dynamo constructed without any iron at all, and, although it would be very inefficient, it would illustrate the principle better than if the iron were there. Our methods of designing dynamos and motors have been very carefully worked out at the present time, so that if we spin the armature of a dynamo we can recover as electrical energy from it fully ninety per cent. of that supplied by hand, or water wheel, or steam engine; and with a motor we can get back ninety per cent. again in electrical energy, if it suits our convenience. We have, then, very excellent means for transforming mechanical into electrical energy and the reverse. The question that is the real subject of my lecture this evening is: What advantages can we gain by employing this double transformation of mechanical energy to electrical and then back again? In other words, when will we be the gainers by this process, in which there must necessarily be some losses. What compensating advantages can we find?

As sources of mechanical power we principally depend on steam engines, or on water-power; various other sources of power, prime movers we may call them, are now and then employed, such as gas engines, that have proved singularly convenient for some small work; petroleum engines, of much



the same general character; hot air engines, used principally for pumping; water motors; wind mills; and a few others. None of these, however, can compete on a large scale with the steam engine, or with natural water-power. Remembering this fact, that from steam or from water we must get our original supply of power, whether we use it directly or change it into the form of electrical power, let us look around and see under what conditions we can generate electrical current, use it in turn to produce mechanical power again, and be the gainers by so doing. One fact is really self-evident at the start; unless a given amount of power can be generated more cheaply at the point where the dynamo is driven than where the power must be finally utilized there is no direct economy in using an electric motor. In fact, the original power must be something like twenty per cent. cheaper at the dynamo than at the motor before the latter can compete directly with steam. I shall only consider steam or water driven dynamos as sources of electricity since all other methods of obtaining it are enormously more expensive; there is, in fact, no other practical way of getting electricity in quantity at all. We hope that there may be some day, but there is none now nor do we know in what direction to look for it; we hear rumors now and then of electricity obtained direct from heat, or directly from chemical energy, but it has been shown over and over again that such methods, although occasionally convenient, are immensely more expensive than the usual one.

Now, under what circumstances can we use the electric motor and gain by it? To answer the question on the moment, there are three general cases where the motor is distinctly economical. (1) When power is ultimately to be used in small amounts and we can take advantage of the cheapness of production on a large scale. (2) Where large amounts are needed but from accidents of situation power can be obtained much more cheaply at some distant point than at the place where it is desired for use. (3) When for any reason it is laborious and inconvenient to produce power at the point of consumption. The first case is an



exceedingly common one. Of a thousand places where power is wanted not less than 900 of them require it in very small amounts, less than a horse-power. A little power is needed to run a lathe here; in the next block a horse-power or two to operate a printing press, on a contiguous corner as much more to drive a bread mixer in a bakery; on the floor above it a half horse-power to drive a dozen sewing machines, and so on. Now we cannot produce power cheaply on a very small scale any more than we can most other things. In fact, the steam engine is singularly ill-fitted for use where the power required is but little. It is an unfortunate fact that steam engines are not usually well made in the smaller sizes. It is a very difficult matter to get an engine of less than about ten horse-power that will be as complete in all its parts, and will compare at all in mechanical perfection with larger sizes. With the electric motor the case is very, very different; an eighth horse-power motor may be as completely worked out in detail as one of a hundred times its power, and may be only slightly less efficient. Beside this, small engines are notoriously and unavoidably inefficient. All the sources of loss have a greater relative value as the size of the engine diminishes.

To pass from the general to the concrete, we can obtain a horse-power for an hour, that is, power enough to raise nearly ten tons 100 feet high in that time, by burning under our boiler about one and one-half pounds of coal, provided we are using an engine of the most approved type and of something like 500 or 1,000 horse-power. With smaller machines, of five horse-power or less, seven to ten pounds of coal would be required to do precisely the same work. So we can see at once that the outlay for fuel alone would be five to six times as great with many small engines as with a single big one, and besides, while the latter can be run by a very few men, the time of forty or fifty would be taken up in caring for the 100 small engines accomplishing the same work. Of course, not all the power generated in an engine that drives a dynamo reappears in the electric current; there are small losses all along the line of operation. The friction of the engine alone frequently

amounts to ten per cent. of its full power, and if it is running at half load we can see at once that twenty per cent. of its power would be consumed in friction, because as the load varies, the friction remains almost constant, being determined by the size and character of the working parts. Then, too, there are small, but unavoidable losses that come from heating the wires, from stirring up currents in the metal work outside of the wires from which we obtain it, in friction of the bearings in which the armature turns and in doing magnetic work on the revolving core of the armature, which is made up of thin discs of sheet-iron impaled upon the shaft. Taken together, all these losses amount to about ten per cent. of the full output of the dynamo, and all of them except one—heating—remain nearly constant whether we are taking much or little current from the machine. Consequently, if the dynamo is worked at only half its full power, these losses become of importance. Altogether, for each horse-power generated in the engine, one is doing very well to get back four-fifths of a horse-power in electrical energy from the dynamo. Still, taking into account all this, if we work on a large scale we can produce electrical energy on the whole very cheaply. It is not a difficult matter to figure out quite exactly, knowing the cost of coal, wages paid to employés and cost of plant, the net cost of each horse-power hour produced. A convenient figure to remember is that with a total capacity of 1,000 horse-power, not less than nine-tenths of it being regularly utilized and working twelve hours per day, the cost of power in the form of electrical energy on the line is almost exactly one cent for each horse-power hour; this includes everything—fuel, attendants, interest on the plant, depreciation of engines and dynamos, oil, waste, and water. In a 100 horse-power plant, working under just the same conditions, the cost would be a trifle over three cents. So you can see at once the advantage of working on a large scale in producing power, and it is evident that even if there were considerable losses in distributing it by means of electric motors, there would still be a very considerable advantage in so doing. If the 1,000 horse-power station of which we

were speaking is only used at a small portion of its full capacity, as I have just shown, some of the losses will be relatively much larger and the cost of power will be considerably higher. If, for example, only a quarter of the 1,000 horse-power were, on the average, to be used, the cost per horse-power per hour would be very nearly double that which I just mentioned. Now, it is very easy to see that electric power under these circumstances is much cheaper than using small steam engines at the places where the power is wanted, even allowing a large profit to the company that supplies the electricity—as a matter of everyday practice a common charge for running electric motors is \$6 per horse-power per month; that is, less than twenty-five cents a day. In taking out, then, a small steam engine and replacing it by an electric motor, there is gain at every point; at the price I have just mentioned five horse-power could be bought for the daily wages of the man who would be required to look out for the engine and keep up the fire under the boiler.

The motor requires only occasional care, takes up very little room, is perfectly under control, and produces no dirt, dust, smoke or steam; and we must remember that with the engine there goes a boiler and a fire, both taking up room and requiring care, and somewhat objectionable on the score of safety. Where power is needed on a very small scale, a quarter or half a horse-power, the advantages of the motor become more and more manifest. No one would think of building or operating in a practical way a half horse-power engine, and if so small an amount of power were required, one would be driven to using a gas engine, or a water motor, neither of which can compete with the electric motor at the rate just given, unless in specially favorable situations. I hope, then, that I have succeeded in making it quite clear that in the case I have first mentioned, where power is required in small quantities, the electric motor is a very successful rival indeed of steam, because it is possible to produce power very cheaply on a large scale and distribute it by means of motors without excessive loss. Further, the electric motor is cleaner, more

convenient and simpler than any description of gas or heat engine which has yet been devised.

Now, let us take up the second case, where a large amount of power is required, for which a steam engine could be very well used, but where it would be rather expensive to operate one; these conditions exist in very many places where it is desired to drive machinery at some point at which it is inconvenient or expensive to get coal. A few miles away, perhaps, may be found an available water-power or a convenient coal mine or coal depot, where the price is low; in very many cases there will then be a decided gain in generating the power where it can be done more cheaply, and transferring the power as electrical energy through wires over across the country to the point where it is needed. Even supposing fifty per cent. to be lost in this transmission—a very large allowance—it often would be very economical. I know, for example, of a mining region in Colorado where power is necessary at each mine to drive stamp mills and other machinery, and where coal would be enormously expensive; five or six miles away from the centre of the mining district is a water-power, a mountain stream shooting downward 300 or 400 feet; this fall is now being used to drive dynamos that in turn will distribute power to every mine that needs it, and some of them are a dozen miles away. In any given case where power is wanted it is no very difficult matter to figure out how much it will cost to run an engine to produce it; and then a counter-calculation on the cost of obtaining the power from a distance electrically will show whether there will be gain or loss by using motors. In some cases, of course, there will be loss, in others, and many, a very considerable gain. So we are quite justified in saying that the electric motor may be cheaper than steam, even when the latter may be used on a large scale; the only condition being that we shall be able to take advantage of cheaper production by the ability electricity gives us to transfer power from a distant point. And finally we come to the third case, where electricity can be a successful rival of direct steam-power—in situations where it is inconvenient or troublesome to get power by any other



means. Fancy, for instance, trying to operate coal-mining machines and drills in deep mines by steam engines placed at or near the point of operation. The difficulties of ventilation and danger of fire simply prohibit successful use of engines, but wires can readily be strung and motors driven at 100 points from a distant power plant, and so we find that electric coal cutters and mining drills are gradually coming into extensive use, superseding the distribution of power by compressed air to a very large extent. Again, there are many situations where to use a heat engine of any description would be exceedingly inconvenient, or would bring down the wrath of the fire underwriters; here a motor can be safely boxed up and run in almost any position and under almost any circumstances with perfect security. Another example of the advantage that comes from convenience may be found in the many ways of using motors about the house to drive fans, to run sewing machines, to operate small pumps, and the like. Always and everywhere it is a power easily controlled, reliable and cheap; it can be led around corners by means of flexible wires, and moved about from place to place with the greatest freedom. It can be brought into play at a touch, and as easily stopped when the necessity for it is past; it can be regulated with the utmost nicety just to fulfil the demands that are to be made upon it, and can be trusted to go about its business almost automatically.

In all this we have seen that we must look upon electricity not as in any sense able to displace steam as a prime mover, but as an enormously powerful and convenient means of transferring power from one point to another with the greatest simplicity and very small losses. Probably for a long time to come by far the greater portion of dynamos will continue to be driven by steam engines, but so far as use at a given point is concerned the electric motor is a most successful rival of the steam engine. If the day ever comes, as it may, although we can hardly dare to look for it, when we can obtain electrical energy directly from coal, as we now obtain heat energy, the days of the steam engine will be numbered. As I have already pointed out elec-

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tricity and heat are very intimately related. Every time we heat a piece of iron, for example, we stir up not only what we call heat, but also electro-magnetic energy, which is the form in which heat is radiated, and if we go on heating long enough we may raise the iron to a white heat, and recognize the energy in its familiar form of light. But all this is merely a dream of the future; perhaps it will be fulfilled some day, but until then we must depend upon our present dynamos.

In all that I have so far said you may notice with wonder that I have not alluded to the use of electricity as a motive-power; to the electric street railway, or to the magnificent possibilities of displacing the locomotive from its present supremacy. I have done this for the purpose of showing first, what can be done with the motor as a stationary machine, and now, of course, you will understand that very much that is true of the motor at rest will also be true of it when it is employed in moving itself and drawing loads. It is merely a question of delivering a certain amount of power at a given point, and it is in this field, really a modification of the second case of which I spoke, that electric power presents some peculiar and almost overwhelming advantages. The ingenuity of inventors has been sorely tried in attempting to build a steam dummy that will perform successfully on street-car lines. It is safe to say that nearly every such effort has come to grief, because an engine under such circumstances is not only working at a disadvantage, but is making itself excessively obnoxious to every one that chances to be on the same street. Here was the grand opportunity of the electric motor, that goes quietly about its business and can be applied at any one point, as well as many, provided it is duly supplied with current. So we connect the motor to the car axle and take current from a line strung overhead, by means of a traveling contact. For street-car service the steam engine is almost out of the question, except as it is used in the cable railroad, so that the motor really does not have it for a rival at all. Young as is the electric railroad the experience of the last three years has been sufficient to show that the horse

must go. Whatever might be the relative cost of an electric road and the same system operated by steam dummies. about the comparison with horses there is no doubt, and the strongest evidence of this is the fact that nearly 300 electric roads are in operation or building to-day, operating over more than 2,000 miles of track. Wherever the change has been made from horses to electricity there has been a positive saving that will grow greater instead of less as improvements are made. The question is often asked whether the electric railroad can rival the cable road; in other words, whether the power can be distributed more cheaply by electric motors or by continuously moving cables. The cable is at the best where the line is short, and generally heavily loaded, and under such circumstances is, at least, as economical as our best present electric railways. The improvements of a few years are likely, however, to reverse these conditions and where the line is long and not heavily loaded, the advantage is altogether on the side of electricity.

PROCEEDINGS  
OF THE  
CHEMICAL SECTION  
OF THE  
FRANKLIN INSTITUTE.

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[*Stated meeting, held at the institute, Tuesday, February 17, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, February 17, 1891.

Dr. WM. H. WAHL, president, in the chair.

Members present: Dr. L. B. Hall, Prof. E. F. Smith, Dr. H. W. Jayne, Mr. H. Pemberton, Jr., Mr. W. H. Bower, Mr. A. W. Allen, Mr. Reuben Haines, Mr. R. A. Fisher, Mr. C. S. Boyer, Mr. H. Warren Shepard, Mr. Everett W. Frazer and four visitors.

Mr. Pemberton nominated Dr. F. A. Geuth as an honorary member of the section.

Dr. Wahl nominated Dr. Hans v. Strombeck, 104 Bank Street, New York City, and Prof. Joseph W. Richards, Lehigh University, Bethlehem, Pa., as associate members. The nominations were referred to the committee on admissions and the candidates were elected.

The treasurer read a letter from the actuary of the institute in reference to the payment of bills for subscriptions; the bills accompanying the letter were referred to the finance committee, which endorsed them for payment.

The treasurer also reported two names to be dropped from the list of members.

The resignations of Prof. R. L. Chase and Mr. Henry Warden were received and accepted.

Dr. Wahl, on behalf of Dr. Greene and himself, made a brief reply to a criticism upon their note on "Alloys of Sodium and Lead," in the *Chemical News*, of January 2, 1891. It was alleged in this criticism that the calculated densities for the alloys, given by the authors, were erroneous, and other figures were stated as the correct densities. The speaker showed wherein the critic had made an elementary mistake, and that the figures given by the authors were right.

Samples of the hydrofluoric acid bottles, patented recently by Prof. Edward Hart, of Easton, Pa., were exhibited.

Prof. Smith presented three papers, entitled, respectively, "The electrolytic method applied to rhodium," "The electrolytic determination of gold" and "The electrolytic determination of mercury."

They were listened to with marked interest and were referred for publication.

Mr. Reuben Haines presented a preliminary paper on the adoption of standards in interpreting the results of water analyses. He considered at length the variability of these standards among different chemists and in different parts of the country. The following figures representing the averages of thirty-four different determinations of uncontaminated waters were recommended as standards for pure waters in the neighborhood of Philadelphia :

	<i>Parts in 100,000.</i>
Free ammonia, . . . . .	'0031
Alb. ammonia, . . . . .	'0044
Chlorine, . . . . .	1'19
Nitrogen in nitric acid, . . . . .	'5075
Total solids, . . . . .	12'57

After some discussion of this paper the section adjourned.

WM. C. DAY, *Secretary.*

## IS SULPHURIC HYDRATE VOLATILE AT THE ORDINARY TEMPERATURE OF THE AIR?

BY GEORGE A. KOENIG.

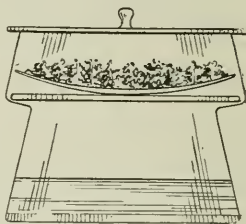
[*Read before the Chemical Section, January 20, 1891.*]

The question proposed in the heading of this communication arose under the following circumstances: I had received a number of skeleton crystals of metallic iron, which had formed in a steel block. To prevent them from rusting, they had been coated with shellac. Previous to the analysis, the shellac was removed by alcohol, and after drying on water-bath, the material not needed for the purpose in view was placed upon a perfectly clean watch-glass into a desiccator of the common pattern. The watch-glass was supported by an iron-wire triangle; its rims did not touch the sides of the desiccator. Upon the latter's bottom was pure sulphuric acid, about one inch deep. The lid fitted with a film of vaseline. The desiccator was then placed upon a shelf and remained there undisturbed for about nine months. Being needed for another purpose, I took it down, and found to my astonishment that both the iron-wire triangle and the iron crystals were covered with a clean white crust. What could this crust be? My first supposition reverted to the nitrous compounds frequently found

even in redistilled sulphuric acid. The crust was very coherent and peeled off in scales, leaving a bright metallic surface. I noticed that those parts of triangle, which had rested in immediate contact with the glass, were not incrustated. Likewise the watch-glass was bright and free from dust. The crust dissolved readily in distilled water, and proved to be composed of *anhydrous ferrous sulphate*. The test for nitric and nitrous acid resulted negatively. A quantitative analysis was not made under the circumstances.

How was this ferrous sulphate formed? Two possibilities only seem to present themselves:

(a) The acid has crept up the side of the vessel, and also from the triangle along the watch-glass, by capillary force. This hypothesis is not well sustained. In the first place



the sides proved themselves dry and clean, and so did the watch-glass. In the second place, one should expect the strongest action, where the triangle rested on the glass. But the fact obtained that these contacts were not corroded.

(b) The acid is volatile at ordinary temperature, similarly to other liquids with high boiling point, such as mercury. It might even be proposed that this volatilization is induced by the strong basic character of metallic iron; and the minute quantities volatilized, being immediately absorbed by the iron, there would not be any appreciable tension. There must have been produced, of course, a corresponding quantity of hydrogen, which would cause tension. I neglected to collect at once the gas in the desiccator and a test was thus impossible. On the other hand, the lid was not so tight, but that so light a gas could not readily escape.



There seems to me at present no escape from admitting the volatility of sulphuric hydrate at temperatures not exceeding  $30^{\circ}$  C., and more particularly in the presence of bodies exerting chemical attraction.

It will be probably of use to many to bear this fact in mind, when they employ sulphuric acid as a desiccating medium. I propose to institute—and, in fact, have already instituted, several sets of conditions, for proving or disproving this volatilization. They will necessarily be time-taking, as time seems the chief factor in the case.

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## CORRESPONDENCE.

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### ON THE OCCURRENCE OF AMBERITE, AMBRITE, OR FOSSIL GUM IN A COAL-SEAM AT HAWAKAWA COLLIERY, BAY OF ISLANDS, NEW ZEALAND.

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[Presented at the stated meeting of the Section, held January 20, 1891.]

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U. S. S. Mohican, OPUA, BAY OF ISLANDS,  
NEW ZEALAND, September 13, 1890.

*To the Chemical Section, Franklin Institute :*

GENTLEMEN :—I have the honor of presenting to the Section, a specimen of coal from the Hawakawa Colliery, Bay of Islands, New Zealand.

This specimen I obtained at the mine, and for this detailed report of its characteristics, I am indebted to Mr. T. P. Moody, M.E., etc., member of the Geological Society of Australia.

*Subject*—"On the occurrence of amberite, ambrite, or fossil gum in a coal-seam at Hawakawa Colliery, Bay of Islands, New Zealand."

"I desire to place on record in the annals of our society a brief description of a most peculiar substance, which is often met with during the progress of coal-getting at this colliery. I refer to 'amberite' or 'fossil gum.'

"This is a gum or resin (mineral), and like most bitumens, it contains upwards of ten per cent. of oxygen, and, also, nearly the same proportion of hydrogen. It bears a close resemblance to Kauri gum.

"This peculiar substance, namely, amberite, imbedded in a coal-seam, has never been seen anywhere, but at the Bay of Islands, at Waikato and at Uhangarei, in the colony of New Zealand.

"In association and intermixed with these coals, especially at the Bay of Islands, large quantities are obtained. It frequently occurs in large masses, but generally is found in layers of small extent, and in 'nodules' extensively imbedded and disseminated over the coal face.

"It is thus described: Its hardness may be represented by two; specific gravity, 1.034; lustre, greasy; color, yellowish-gray, sometimes light and dark green and occasionally encrusted with a 'film' of carbonate of lime and spar; subtransparent; fracture, conchoidal; its chemical composition:

Carbon, . . . . .	76.88
Hydrogen, . . . . .	10.54
Oxygen, . . . . .	12.77

"It is wholly insoluble in alcohol, ether, oil of turpentine, benzole and chloroform and dilute acid. It burns with a yellowish smoking flame. The ash contains iron, lime and soda.

"This amberite is an exudation from the Kauri pine tree, changed by chemical action, the only difference between it and Kauri gum being in color mainly. This amberite occurs in one seam of coal which belongs to the cretaceo-tertiary formation. In this series here there is only one seam of coal.

"The secondary formation of other countries, is, in this place, entirely absent, therefore the cretaceo-tertiary formation necessarily reposes upon the primary, viz: 'diorite slate.'

"In order that comparisons may be made as to other products somewhat bearing a resemblance to amberite, I give a few particulars of *re* gums or resins which are found in Europe.

"For instance, the amber of the Baltic Coast occurs in irregular mains, without cleavage. Its hardness is 2.25; specific gravity, 1.065 to 1.081; lustre, resinous; color, yellow; it is, however, sometimes reddish, brownish and whitish, often clouded; streak white; transparent; tasteless; electric on friction. Composition:

Carbon, . . . . .	78.824
Hydrogen, . . . . .	10.228
Oxygen, . . . . .	10.900

"Then, again, the substance well-known to scientists as 'geomyricite.' This in color is 'wax-like' white; melts at 80° to 90° F.; is easily soluble in hot alcohol and ether, but slightly only in alcohol of 80 per cent. This is thought to be the resin exuded from some palm tree. Its composition:

Carbon, . . . . .	80.59
Hydrogen, . . . . .	13.42
Oxygen, . . . . .	5.99

"This substance is almost identical with 'China wax.'

"In concluding I would briefly remark that all the gums referred to are, according to common opinion, the exudations of trees that flourished at the time the deposits were forming in which these substances are found."

Before closing I will state my experience with this coal as fuel for steaming purposes.

I have used this coal in the furnaces of the boilers of the United States steamer *Mohican* and find it to be a good coal for that purpose. Combustion is more rapid than with any other bituminous coal I have ever used.

It does not make any ash, but deposits a cinder or clinker upon the surface of the grate bars. This deposit is easily removed, as upon opening the furnace door to manipulate the furnace, the cold air being admitted, the cinder immediately detaches itself from the bars and is removed in the ordinary way. This cinder (a specimen of which I send) amounts to about eight per cent. of the coal consumed in the furnace. The smoke is of a light pearly-gray color.

Very respectfully,

J. M. EMANUEL,

*Passed Assistant Engineer, U. S. N.*

## BOOK NOTICES.

*Force and Energy, a Theory of Dynamics.*—By Grant Allen. London: Longmans, Green & Co., and New York: 15 East Sixteenth Street. 1888.

In the preface to this very interesting work, the author states that "the perusal of certain dynamical treatises of Clerk Maxwell's, Tait's, Balfour Stewart's and Helmholtz's, suggested to his mind sundry profound difficulties in the current conception of the nature of energy;"\* and he proceeds, with the utmost modesty, to suggest an entirely new theory, or new general view, of the mechanical processes of the universe.

Those who feel an interest in questions of what may be called ultimate dynamics, and who are not over-ready to cry "heresy" upon the appearance of every-proposed innovation, will follow with pleasure the steps by which the author goes on to show how his theory harmonizes with those processes. The work is of special interest in connection with Prof. Lockyer's recently published views respecting the evolution of the universe through the aggregation of meteoritic dust.

Unfortunately, the new theory requires a new nomenclature, assigning new or modified meanings to terms already used, as will appear if we briefly summarize the author's propositions, beginning by quoting the opening paragraphs of the first three chapters, on "power," "force" and "energy," respectively.

"A POWER is that which initiates or terminates, accelerates or retards motion in one or more particles of ponderable matter or of the ethereal medium."

"A FORCE is a power which initiates or accelerates aggregative motion, while it resists or retards separative motion, in two or more particles of ponderable matter (and possibly also of the ethereal medium)."

"An ENERGY is a power, which resists or retards aggregative motion, while it initiates or accelerates separative motion, in two or more particles of ponderable matter or of the ethereal medium."

The "Forces" (or "Aggregative Powers") of nature, are the molar force of gravitation, the molecular force of cohesion, the atomic force of chemical affinity and the electrical force of unlike electric units.

\* It seems most unfortunate that the author has not even given us a hint as to what these difficulties were.

"Every Energy (or "Separative Power") in the universe was primordially a mere statical separation of masses, molecules, atoms or electrical units. If there were no such power as force, every one of these bodies would have remained forever apart and immovable. But as forces draw together these mutually attractive material objects, their energy assumes for a moment the kinetic mode. The falling water, the moving glacier, the oxygen rushing to unite with the coal, each pass,\* for a shorter or longer period, through the kinetic stage. As they aggregate, their energy is given off in some other form of motion, involving separation. But as they move about, they part with this motion in separating other masses or molecules, and the attractive force soon brings them together again."†

Energy, like force, is classified in the four "species:" molar, molecular, atomic and electrical, accordingly as it is exerted between masses, molecules, atoms or electrical units. It is also recognized under two modes, potential and kinetic. Finally, since motion may be either separative (as when a cannon ball is fired upward), aggregative (as when the ball falls again), or neutral (as in a spinning top or in a planet circling about its sun), so kinetic energy may be either of the separative, aggregative (notwithstanding the definition in chapter iii) or neutral kind.

But every such transaction is accompanied by a transfer of energy from matter to the ether. This energy, mostly in the form of heat, is radiated in all directions into space. Some small portion is necessarily intercepted by such of the heavenly bodies as it meets here and there; but the net result is a continuous transfer of energy from matter to the ether; in other words, a continuous waste of energy, so far as concerns man's uses. For even the small portion of radiated energy, which is thus intercepted by bodies in space, is only temporarily retained by them, and is sooner or later again passed on by them to the interstellar spaces.

This transfer of energy from matter to the ether is always accompanied, as a final result, by a further aggregation of matter. "The life-history of the material universe, as known and predictable by us, is the history of its passage from a diffused nebulous state to an aggregated solid state, through a vast number of intermediate stages, each one of which is an advance in aggregation upon the stage which preceded it."‡

The author traces this process of transference of energy and aggregation of matter, through sidereal, solar, terrestrial and organic phenomena, drawing many and apt illustrations from each of these spheres.

The orbital energy (our "centrifugal force") of the planets resists the (centripetal) force of gravitation, which would otherwise draw them to the sun. But this orbital energy is constantly being expended in, and reduced by, ethereal friction.||

"Every particle of matter has inherent in it certain forces of which it can

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\* Rather the energy of each passes.

† Page 76.

‡ Page 89.

|| The author is careful to state that while *forces* (*i. e.*, attractions) may or may not be exerted between the particles of the ether, those particles are certainly endowed with *energies*.

never be deprived. The total amount of force, or aggregative power, in the universe is thus always a fixed quantity. This principle may be known as the persistence of force.\*

"The total amount of energy, potential and kinetic, existing in the Universe is always a fixed quantity."† But while force is "rigidly bound up with the individual particles in which it is from time to time manifested," energy "can be transferred from one particle, or set of particles, to another." Hence while forces are spoken of as "persisting," energies are said to be "conserved." These two generalizations are summed up under the wider one of "the indestructibility of power," thus: "the total amount of power, aggregative or separative, in the universe is a constant quantity, and no power can ever disappear or be destroyed."

One force or energy may be prevented from producing its aggregative or separative motion, by the interference of another force or energy; as when gravitation is prevented from drawing a weight to the earth by the cohesion of a string or shelf supporting the weight, or as when the energy of steam in a boiler is restrained by the cohesive force of the particles composing the boiler shell. Such cases our author treats in chapters xi and xii, under the heads "the mutual interference of forces," and "the suppression of energies." But such interference may be terminated, and motion permitted, by the action of a "liberating energy," as when the energy of heat burns away the string or shelf which supports a weight and thus permits the latter to aggregate with the earth. Such liberation may also evidently be effected by a liberating "force," as well as by liberating energy, although this is not stated by the author. Thus, the shelf supporting a weight might be *pulled* away by the attractive force of a magnet, or by that between another weight and the earth, acting through a cord passing over a fixed pulley, as well as *pushed* away or burned away by energies.

*Vegetal* organisms, during life, act chiefly as *accumulators* of energy; for they absorb water and carbonic acid; decompose them into their constituent elements, or into "energetic compounds" (such as hydrocarbons), and retain those elements, etc., in a state of separation or potential energy, using only small portions of this energy (transforming it into the kinetic mode) in germination, efflorescence, etc. By decay after death, or by absorption into some animal organism, the stored-up potential energy is kinetized, liberated and turned out upon the ether, and the previously separated elements are allowed to recombine.

*Animal* organisms, on the contrary, are *expenders* rather than accumulators, of energy. They devour the "energetic compounds" (hydrocarbons, etc.) evolved by vegetal organisms, and thus cause them to evolve kinetic energy (forming part of the life of the animal, and afterwards going to waste), and to aggregate by recombining with oxygen to form stable or non-energetic compounds.

It thus appears that what we commonly call the "*de*-composition" of organic matter, is more properly described as *re*-composition; for it consists in a

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\* Page 39.

† Page 41.



reuniting (under the force of chemical affinity) of atoms of carbon and hydrogen, with oxygen from which they have previously been separated by solar energy acting through the organism.

The foregoing is, we believe, a fairly complete outline of the new theory.

A radical defect of the present treatise, and one which prevents it from doing justice to the theory, is the entire absence of the quantitative feature. We are left quite uninformed as to the *dimensions* of the author's "forces" and "energies." Our old-fashioned "force" may be measured in pounds, and our "energy" in foot-pounds; but we are not told in what units these new "powers" are measurable, or even whether they are or are not both measurable in one and the same unit. On page 11 we are told that the amount of the energy required to lift a given weight to a given height is the measure of the resistance offered by gravitation. From this it would appear (if, as we generally understand, "resistance" is the equivalent of "force") that the "force" of gravitation is to be measured in *foot-pounds*; but, if so, how are we to measure the force of gravitation acting between the earth and a stone resting upon it; and how are we to understand the statement, already referred to, that "every particle of matter has inherent in it certain forces, of which it can never be deprived?" In several places, too, the author speaks of kinetic energy as being equivalent to motion. Is motion, then, to be measured in units similar to the foot-pound?

We are inclined, notwithstanding the author's explicitness in general, to attribute the occurrence of these somewhat puzzling expressions to an occasional, and by no means unpardonable, lapse in the matter of rhetorical accuracy; for instance, in several places we find *mist* referred to as a form of *energy*, whereas probably no one knows better than our author that it is a form of *water*.

There are many cases of every-day occurrence in which it seems difficult to decide how to apply the author's distinction between "force," as aggregative power, and "energy," as separative power. For instance, when a man lifts a dumb-bell from the ground to himself, he no doubt does so by means of "energy," for which he is indebted to the chemical aggregation (within his body and under the action of the force of chemical affinity) of carbon and oxygen which had previously been separated by solar energy, acting through the medium of vegetal processes. And in lifting the dumb-bell he is plainly counter-acting the "force" of gravitation between the earth and the dumb-bell, and separating them in spite of that force. Yet, on the other hand, the man's energy manifests itself primarily as a *contraction* of his muscles, or as an *aggregation* of their particles, for which, by the author's definitions, we require, not energy but force. Besides, while he is separating the weight from the earth, he is drawing it toward himself, or causing aggregative motion. Have we here then a case of (animal) energy overcoming the force of gravitation, or are we to regard it as a case where one force (gravitation) is overcome by another (muscular attraction)? And how must the answer to this be modified for the case where the man, after *pulling* the dumb-bell from the ground to the level of his shoulder, extends his arm and *pushes* the bell both from himself and from the earth? Even here, we must remember, the extension of the arm

and the further separation of the dumb-bell and the earth are produced by the *contraction* of muscles, viz: those at the back of the arm.

Except for the necessary difference in nomenclature, it does not clearly appear wherein the theory here advanced conflicts with those usually held. Each is but a method of describing and explaining occurrences upon whose actuality all are agreed; and while two or more such methods may be correct, that one is the best and the most convenient, which most completely harmonizes with those occurrences and leaves the fewest phenomena unaccounted for. In the absence of quantitative data it would seem quite impossible to institute any satisfactory comparison in the present case.

It is worthy of note that the new theory has been made by Mr. Edward Clodd the foundation of his "Story of Creation,"\* a popular and compendious, but serious, exposition of the doctrine of evolution. Mr. Clodd emphasizes the convenient distinction between energies as *pushing* powers and forces as *pulling* powers, and further notes the more radical distinction between energies as powers that may be stored (since they exist whether they act or not), and forces as powers incapable of storage. T.

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## Franklin Institute.

*[Proceedings of the stated meeting, held Wednesday, February 18, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, February 18, 1891.

Jos. M. WILSON, president, in the chair.

Present, 210 members and thirty-one visitors.

Additions to membership since last report, eighteen.

The Secretary announced the death of Edward Lang, LL.D., F.R.S.E., of Edinburgh, Scotland, an honorary member of the institute; also the resignation of Prof. Wm. D. Marks, as a member of the Committee on Science and the Arts.

Mr. Otto C. Wolf was elected to fill the vacancy.

The grant of the Edward Longstreth medal of merit to Henry W. Robie, for his improved screw-jack, and to Wallace H. Dodge for his wood split pulleys, was reported.

Mr. Arthur Kitson read a paper on his system and apparatus for generating fuel gas for domestic and manufacturing uses, illustrating the same with the aid of views upon the screen. (Referred for publication.)

Mr. Edmund Sterling exhibited and described a second series of colored lantern slides of Japanese subjects.

Mr. Wm. E. Lockwood offered the following, viz:

*Resolved*, Mr. Wm. B. LeVan assenting, that an evening be specially set apart for the further and fuller consideration of his paper on "High Speeds on Railways," previous to its publication in the *Journal*.

Adopted.

The Secretary's report embraced a description (with exhibition of specimens) of an improvement in Argand gas burners invented by Mr. A. G. Morey; and a bottle formed of a mixture of ozokerite, paraffin, etc., designed by Prof. Edward Hart, for holding and transporting hydrofluoric acid. He gave an account of the organization and present condition, condensed from the most recent reports, of the work on the Nicaragua Inter-oceanic Canal, and a brief *résumé* of the progress made within the past few years in the manufacture of aluminium and its alloys.

The Secretary announced that the amendments to the by-laws, relating to the creation of a class of members to be called non-resident members, had been duly advertised in the daily papers as prescribed. The amendments were, thereupon, adopted without dissent.

Mr. Wm. B. LeVan offered the following amendment to Article VI of the by-laws, to-wit: the addition thereto of a fourth section, to read as follows:

SECTION 4. No member shall be eligible for re-election to the office of manager, or be a candidate therefor, while he is filling such office, nor sooner than twelve months after the expiration of his term of office.

The subject evoked an extended discussion, the outcome of which was the adoption of the following resolution, offered by Mr. Spencer Fullerton, viz:

*Resolved*, That the subject of the proposed amendment be referred to the board of managers, with the request for an expression of opinion as to its advisability, and that the Secretary be directed to print the text of the proposed amendment, together with the action of the meeting thereon, in the bulletin of the March meeting.

Adjourned.

WM. H. WAHL, *Secretary*.



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THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the journal.

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## THE PROGRESS OF CHEMICAL THEORY: ITS HELPS AND HINDRANCES.

BY DR. PERSIFOR FRAZER, Prof. of Chemistry.

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[*Introductory lecture to the chemical course delivered before the Franklin Institute, November 21, 1890.*]

It is the ordinary reproach to science of the ignorant and disingenuous, that its conclusions at any given period are no more stable than the wildest speculations of the fanatic and dreamer.

We read continually in the papers some arrant nonsense said to have been pronounced by "one of the eminent scientists" of such-and-such a place and time, which the course of events has disproved; and the public is left to the conclusion that the gains of science are only air castles certain to dissolve when they become unpopular, and certain to lose popularity when the first pleasurable effect of the announcement has passed away. The following extract, from the *St. Louis Republic*, will furnish a case in point:

"It has only been sixty years since a great mathematician demonstrated that a steamship could never cross the Atlantic because it would be impossible for her to carry enough fuel to last during the trip. Before he had hardly deduced his calculations a steamer from America glided into port."

The name of the eminent scientist is not mentioned, and it is safe to conclude that if any man made such an observation he either would have failed of recognition by the class to which he is said to have belonged, or he was false to the fundamental principles of inductive science. It is not the province of inductive science to establish what is impossible, but what is in various degrees likely. Its premises are facts and its conclusions are probabilities; in many cases weak, but in others so strong that they produce the same effect upon the mind as certainties. Nor is it true that the gains of science are evanescent. Parallel with the accumulation of observations run the generalizations upon them. These generalizations are usually passed through the purgatory of hypothesis before they attain the bliss of theory, but no theory is old enough yet to have become more than a theory, though some have stood so many tests of their truth as to carry the conviction of axioms.

In looking over the histories of the sciences one finds the same general course of progress. At the outset in the halcyon days of the old Greeks, it is likely that some wise words will be found to have been spoken concerning them all; words that astound us with the apparent insight they show into problems which it would seem that the last twenty-five centuries were needed to give. But the centuries were not all equally productive. There came across the path of every systematic study of the laws of nature, first the cultured blight of the Aristotelian philosophy, which assuming to know everything, in fact largely contented itself with verbal jugglery, whereas accumulation of facts was the only road to knowledge; obliterating the forward steps that had been made, and substituting in their place the evolution of the universe and its laws from within. It was a philosophy where the distinction between words and things was

obscured, and a natural fact was attained by means of a pretty syllogism.\*

Vastly worse were the centuries which followed, known as the middle ages. Centuries of ignorance, selfishness, and crime; when the possession of any knowledge but that of an armorer was looked upon with distrust and ascribed to the devil.

The different natural sciences emerged from this barbaric condition one to three centuries ago, and under the liberty of enlightenment, with the stimulus of more general education; have attained an abode in the life of the race from which it is difficult to see how they can be displaced without such a general cataclysm as would nearly destroy the human race itself.

Amongst these sciences, that of chemistry has had such a marvellous career that it is, perhaps, the best example which could be selected of the progress just alluded to. It illustrates aptly not only the methods employed in building up an inductive science, but the things that have helped and those that have hindered a development which, nevertheless, in spite of all hindrances, must fill us with a sense of wonder.

Our reason for this is that from various causes the real growth of chemistry only began in the seventeenth century, and that even then it lost nearly a hundred years in the quagmires of a false hypothesis which not only directed the efforts of chemists into unfruitful fields, but destroyed the value of the conclusions they reached from their work. Yet, even with all these drawbacks, no domain of human investigation has been widened so rapidly and with such advantage to the world.

At the very outset of the subject we find a generalization of old Democritus (who lived 450 B.C.) so astounding in its character and so accurate in most of its statements that only in the past few years have chemists been able to reach these profound thoughts thrown across the ages into the

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\* It is intended to refer here to the exposition of the Aristotelian philosophy, by its disciples from about the time of the Christian era to the eighteenth century, and not to disparage the marvellous genius of Aristotle.

midst of the civilization of our time, as the legend has it that Bruce's heart was thrown by the Douglas into the hands of the Saracens to stimulate the ardor of the Christian Knights to charge and recover it. But, unfortunately, no such effect was produced by the good old laughing philosopher; though at a snail's pace, and after a lapse of 2,300 years we have reached the spot. Briefly, as transmitted to Epicurus, and expanded by Lucretius B.C., 99-55, it was this.\*

The universe consists of atoms and space. The atoms are of many forms and of different weights, and the number of atoms of each form infinite. Change is only the combination and separation of atoms! Atoms are in constant motion. "First beginnings" or atoms are never destroyed or worn out. The difference between a hard body like iron, and a soft body like air, is that in the first the atoms move to and fro within small distances; in the soft body they move freely or rebound from each other only at long intervals.

Bodies are partly "first beginnings," partly unions of "first beginnings." The properties of the bodies formed of the groupings of "first beginnings" need not be like the properties of the "first beginnings" themselves. "It matters much with what others and in what positions the first beginnings of things are held in union and what motions they do mutually impart and receive."

These views are extraordinary, and, with the exception of the difference in the form of atoms, which is a point beyond what we have been able to reach even now, the above contains a very fair statement of the atomic theory which is held by the most advanced chemists to-day.

How Democritus could have reached such conclusions is a mystery, but his annunciation of these recondite truths very well illustrates the fact that an hypothesis, be it never so beautiful and even true, if unaccompanied by facts to support it in no way helps the progress of natural science. Like every other guess it indicates merely the frame of

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\* *Democriti Aderitæ operum fragmenta.*

mind of the man making it. It is like a floating shadow on the sea of time. Perhaps it defines substance, perhaps only a cloud of fancy.

This seed thrown off by Democritus found no soil of facts on which to grow, from his time until late in the present century, although Gassendi, Canon and Provost at Digue, in France, after ages of ignorance, proposed it again, but without proof; and it is thought to have influenced the minds of Newton and Boyle.

This then is one example of an occurrence in the history of the science which to all appearance neither helped nor obstructed its progress unless in the indirect way of teaching men's minds to grasp large and comprehensive thoughts. All could not have been ignorance and degradation in Abdera (Thrace), or Miletus, or Athens, where a language existed capable of conveying from mind to mind thoughts like these, and where a mind was capable of conceiving such thoughts.

It teaches the student of natural history a lesson in addition to that of the old traveller's speculations, and it may serve to illustrate the difference which the late Prof. Clifford of Cambridge pointed out between accepting those conclusions of natural science which one has been taught, but has not personally investigated, and accepting what is said to have been revealed, but which, it is acknowledged, is not susceptible of any proof. In the one case the way is open to any one to pursue any single direction which has been before taken; measuring and judging of the correctness of the steps of one's predecessor; but in the other case there is no path anywhere, and the correctness of the position assumed cannot be judged. It is the difference between, on the one hand, handing the keys of a hundred trunks to a custom house inspector, who has at best time to examine but one or two, asking him to satisfy himself of the accuracy of your description; and, on the other, telling him that something indescribable ought to convince him more thoroughly of the contents of the trunks which he cannot inspect, than of those which he can. Speaking generally it may be said that



a proposition of which the steps which led to its acceptance cannot be indicated and followed, has no place at all in the domain of science, though it may be true.

Such propositions were those of Democritus above given and it is quite just that in the absence of logical proof they should have been excluded from the realm of science, and that to him who first showed reason for believing them should be accorded the honor of their discovery.

Of much less importance is the next hypothesis of the nature of things which we find annunciated by Aristotle in his quadrilateral of states: solid, fluid, dry (or warm), and moist (or cold), or what he supposed to be the elements of all bodies, viz: earth, air, fire and water. It was unfortunate, and yet in accordance with the usual march of events that this utterly inadequate and narrow guess should have fettered men's minds for 2,000 years, owing to the mighty hold which Aristotle took of all nations.\*

As his historian remarks, Aristotle's works had a prodigious influence in Asia, and Europe, and Africa; among the Persians, Arabs and in Germany where part of his ethics were read in the churches on Sunday instead of the Bible. In the middle ages, too, these elements of Aristotle were imbued with a mysticism more than Platonian.

It was the spirit of that middle age when the ignorant classes being the powers, made patient scientific work difficult and dangerous, that learning was concealed under the mask of paradox and cryptogram as if it were a crime. Whatever Aristotle's view of his elements may have been, it took a new direction, beginning with Geber in the eighth century.

The first chemists were alchemists who sought the transmutation of base metals into gold; the philosopher's stone; and the elixir of life. These were represented by Geber (an Arabian alchemist of 760), Albert von Bollstädt (1193-1280), Roger Bacon (1214-1294), Raymond Lull (1235-1315), Arnald de Villanova, Caletonia (1235-1314), etc. Those who examined physical problems retained the Aristotelian view,

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\* See Aristotle, *A chapter from the history of science*, Lewes.

while the alchemists took more or less modified forms of Geber's doctrine, that the metals were composed of mercury and sulphur. As an instance of the confusion which reigned in the ideas of this time, some believed that these constituents of metals were real sulphur and real mercury, while others believed that qualities were intended by these terms. Geber ascribed to the sulphur the property of giving different colors to the metals.

At the end of the fifteenth century the alchemists had added salt to mercury and sulphur. Many regarded the Aristotelian elements as the ultimate; and mercury, sulphur and salt as the intermediate or proximate elements, as, for example, Basil Valentine, who extended the number of substances of which these were the ultimate elements, from metals to all known matter, but denied that they were the common substances which we know under their names.

In the early part of the sixteenth century the failure to find the philosopher's stone led to the decadence of alchemical or transmutation chemistry, and the rise of iatrochemistry or that of healing. Paracelsus (1493-1541) taught that in a burning body the sulphur quality represented the inflammability, the mercury the sublimation, and the salt the ashes.

From this to the end of the seventeenth century disputes as to tenets were numerous, but no real progress was made, Agricola (1490-1555) attacked Paracelsus and fell back upon Aristotle. Libavius wrote the first treatise on chemistry (1595). Van Helmont (1577-1644) denied all Paracelsus' views and sought an universal solvent, which should be a panacea. He first recognized the existence of gases and quantitative relations, and opposed Aristotle's doctrines that fire was a body or earth an element; but believed water and air were such.

Glauber (1603-1668) though possessing variable views, invented better means for separating bodies. Sumert (1572-1637); Willis (1621-1675); Lemery (1645-1715) believed in five first principles—mercury (spirit), sulphur (oil), salt, water (phlegma) and earth. Lemery taught that these were in rapid motion, and thus gave rise to the obvious proper-

ties of things. He explained the well-known phenomenon of the calxes of the metals weighing more than the metals themselves, by supposing that in burning they absorbed fire material.

The real philosophy of chemistry commences with Robert Boyle (1622-1691), who denied the accuracy of the doctrines both of Aristotle, and the later alchemical and iatro-improvements upon them. He believed that heat had not the power to transform complex substances into their constituents, but on the contrary, sometimes produced complex out of simple substances, and sometimes was without effect. Other agencies than heat could produce the same effects. He strongly denied that one could predict the number of simple substances as Aristotle and his successors had done. He thought it probable, however, *that the so-called elements consisted of the same kind of matter*, differing only in the size, form, etc., of their respective smallest parts.\*

It is well to pause for a moment here to consider these logical and scientific views of Boyle, not alone because they introduced for the first time a rational inductive system of chemistry, emancipated from the mysticism and superstition of the ancients, but also because they are typical of one of the greatest of helps to the progress of chemical theory, independent and fearless criticism.

Except the brilliant guess that the so-called elements consisted of the same kind of matter, Boyle's mission seems to have been to hew down the weeds and undergrowth which had impeded the march of the science; yet his services were invaluable, as without them no further progress could have been made. This fact illustrates also the injustice of the cry so popular in some cases when the fallacy of a proposition has been exposed :

"What have you to set up in its place?"

Surely it cannot be required of him who discovers a flaw in a supposed explanation that he should be always ready with a sound explanation. The two characters of mind,

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\* See Kopp's *Geschichte der Chemie*.

which are required to accomplish these very different tasks, are entirely unlike.

Plato and Aristotle probably regarded the lightning stroke as a natural phenomenon, and could have refuted the popular belief that it was the missile from Zeus' hand, but it required dozens of centuries of observation before even the most remote approach to an explanation of the phenomenon could be given.

As soon as the ground is cleared of rubbish, other and more rational theories have a chance to grow. Therefore, the iconoclast, if impelled by his sense of truth, and if considerate in his methods, is a necessary pioneer and axeman ahead of the great army of science. It is so much easier, however, to throw down than to build up, that the iconoclast business is often overdone by those who are incapable of any more skilled service to science, and who confound the art of attacking everything with the duty of overthrowing evil. All honor to Robert Boyle for calling a halt in the unbridled fancy of the chemists of his day and clearing the way for a new era! All honor to his deep insight into the workings of nature, that he announced independently what old Democritus had dimly foreshadowed 2,000 years before: and what it was reserved for a great chemist now living to put in words and carry almost to the state of an accepted theory: yet to neither of them will belong the credit of demonstrating the unity of matter, but to some one, it would seem, who shall pass the speculative stage and offer proof. It looks as if this were not to be long delayed.

Both by Boyle's destructions and by his conceptions he aided the progress of chemical theory, as few have done since his time, and chemistry or the study of the most intimate relations of matter, as distinguished from alchemy, magic or the healing art may fairly be said to have started with Robert Boyle.

Singularly enough the first sapling to spring up and occupy the new clearing made by Boyle was an error so gross that it seems to the youngest student of to-day grotesque in its clumsiness, and yet defended by some of the subtlest of sophists, it took 100 years to overthrow it.

And the most instructive part of its history is that it was finally overthrown by an argument which Boyle himself had employed; which had been employed by other sceptics, and explained away by the phlogistonists, and was ultimately and successfully refuted with the same experimental proof by a countryman of Boyle. It is often the case that an attack in front, over the very ground of numberless previous repulses is successful, and it was the case here as shall be briefly shown.

Stahl (1660-1734) was a physician of independent views, who adopted Becher's theory of combustion or the changeability of bodies by heat. He believed that he had settled experimentally this question :

"Is a common quality present in sulphur and carbon? or is one contained in the other?"

The generally accepted view at that time was diametrically opposite to that which Boyle held of combustion, and might be stated thus: Sulphur consists of oil of vitriol and some combustible body, which latter escapes in burning. Stahl combined oil of vitriol with an alkali and heating the combination with carbon obtained an alkaline sulphide similar to that produced by sulphur and an alkali. From this, sulphur (or vitriol) can be separated.

Therefore, the combustible in carbon and sulphur was the same!

Heating calxes of the metals with carbon, there resulted the metals. The metals were then composed of the calxes and this substance. Fats and oils produced the same effect with the calxes, and, hence, in them too was the same combustible substance.

Stahl called this combustible "Phlogiston."

This hypothesis was rapidly installed into the rights and dignity of a theory and rallied around it as such, some of the brightest and best minds for three generations.

It was not only faulty in its conclusions, but inadmissible in its steps, and should have incurred the opposition of every intelligent man who understood the limitations of inductive philosophy: but it occupied the vacant space left bare by the labors of Boyle, and with a



growth as luxuriant as it was pernicious to the attainment of truth, obstructed in many ways all valuable advance of chemical theory, while it lasted.

It may not be amiss before sketching its rise and overthrow, to point out here wherein its inherent fallacies should have condemned this hypothesis from the outset.

Hypothesis means a guess—a temporary structure erected by the employment of the imagination strictly governed by experience, for the purpose of more rapidly reaching a generalization than by waiting for all the facts which in the end will be necessary to sustain a full-fledged theory. After one or two facts bearing on a subject are ascertained, it often happens that the mind is directed towards the possible existence of a law which would explain them both, but numerous unknown and untried experiments must result in a certain way in order that this supposed explanation may stand.

With time and a constantly increasing experience more and more such facts are ascertained. If all fall into their places the hypothesis grows stronger and stronger in probability until by a large accumulation of such corroborations, the hypothesis passes the undefined line which separates it from theory, and becomes a theory.

This theory then goes on increasing in strength by each additional fact which is found conformable to it, until its convincing force is almost as great to the mind as one of the facts which are the bricks of its construction.

But if during this period of probation of an hypothesis or of a theory a single fact is well authenticated which is inconsistent with it, the hypothesis or theory must be abandoned. Of course, in the case of a theory which had been tried and proven hundreds of times and found to apply to newly-discovered facts, its abandonment would be held in abeyance until every effort had been made to prove the authenticity of the fact and its inconsistency with the supposed explanation; but if these were unalterably confirmed the hypothesis or theory must fall.

This constitutes the true principle of inductive philosophy, and only by pursuing this path rigorously can its processes lead to any good result.

The obvious difficulty of the phlogistic theory was that the calx, which was considered the simple body was heavier than the metal which was supposed to be the compound. and this circumstance was explained by supposing that the phlogiston which was absorbed from the fire when the calx was heated therein had the principle of "levity" as opposed to that of "gravity," and that the more of it which was collected in a body the lighter became that body.

Here was the unpardonable error of the phlogistonites in the defence of their theory.

[*To be continued.*]

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## RIVETED JOINTS IN BOILER SHELLS.

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BY WILLIAM BARNET LE VAN.

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[*Read at the stated meeting of the institute, held November 19, 1890.*]

The high steam pressure which boilers are now carrying render it desirable that the utmost care should be taken to unite the various plates of which they are composed in such a manner that the joints shall not only possess great strength when constructed, but also that they shall be but little influenced by wear and tear.

A riveted joint is, in a certain sense, an imperfect part of a structure. It cannot be so designed as to be strained uniformly throughout. It has always certain surfaces materially weaker than the rest, at which, consequently, deterioration of material, or fracture, by the action of the load, is liable to occur. These surfaces of weakness are so related that the increase of area of the one involves a diminution of area of the other.

The joint, therefore, which will carry the greatest load before fracture will be that in which the stress reaches the breaking limit for each of those surfaces simultaneously. Since the rivet section can in general be increased only at the expense of the plate section in the strongest joint, the rivet and plate will reach their breaking strain simultaneously under the same load.

It would seem, therefore, that the proper proportions or respective areas of a riveted joint could be determined by

the ordinary rules of applied mechanics without the need of experiments. But that this is not so is probably due mainly to a second condition of imperfection in riveted joints. To apply the ordinary rules for the strength of materials to riveted joints it is necessary that the distribution of the stresses on the respective surfaces should be known.

If those stresses were as uniformly distributed, as in an ordinary bar test for tension or shearing, the problem would be simple. But, in fact, the stresses are less uniformly distributed, and the law of their distribution is unknown. Consequently, the average stress on the surface of fracture of a riveted joint, when broken by a load, is less than it would be if the stress were uniformly distributed, and needs to be determined by special experiments. Further, it may be different for different forms of joint. This average stress, always less than the maximum stress which causes fracture, is here termed the apparent breaking stress. Hence, the chief object of experiment on riveted joints is to determine the apparent breaking stresses.

(1) For the different surfaces at which each joint may fracture.

(2) For the different forms of joints.

In certain cases allowances may have to be made for progressive deterioration of a joint, by corrosion or otherwise, which reduces the strength in certain directions more than in others. No experiments showing the amount of deterioration in such cases appear to have been made.

It is well known that the greatest strain upon plates of cylindrical boilers comes upon the horizontal joints, the pressure tending to burst the shell in the radial direction. The pressure of the steam upon the heads and the strain upon the transverse seams or joints is but one-half that tending to rupture the longitudinal joints. Besides this, the heads, in some cases, being dished are additionally strengthened (the extent of the dishing being equal to a radius of the diameter of the boiler shell), and in the case of flue boilers the heads are strengthened by the flues and tee-iron (T) girders on the flat surfaces above the flues connected by rods generally attached to the centre shell plate; but better still when

stayed from head to head connected by turnbuckles, so it will be seen that the weakest points are the longitudinal seams.

In all boiler joints it is important that the rivet holes in the various plates and braces should coincide perfectly with each other, and that they should be completely filled by the rivets. If they do not all correspond accurately they should never be forced to do so by the use of a drift-pin. Rivets should be placed in those rivet holes which agree properly, and the remainder should then be reamed out until the holes coincide.

The strong competition in boiler making, and the ruling low prices at which work is contracted for to be delivered, have caused great carelessness in this industry. In my experience as a boiler inspector, I have found boilers made by parties who stand high in the trade in which the plates were not punched to properly coincide: so little so that they formed eccentric openings to receive the rivets through which a drift-pin had been driven to allow the rivet to enter. The use of the drift-pin is a relic of barbarism; it not only overstrains the boiler plate, but inclines the hole so that the rivet will not be at right angles to the plate. The strength of such a rivet may be *only twenty per cent.* of that of a driven rivet in properly-punched plates. A drift pin should *never* be used. When rivet holes do not coincide they should be reamed until they do, and then rivets of proper size to fill them should be used.

Commercially, boiler-plates cannot be drilled or punched with sufficient accuracy to coincide with one another, therefore, all rivet holes should be punched (say) at least one-sixteenth of an inch less in diameter than the rivet's diameter after being driven, and when all the plates are brought well together in place, by temporary bolts, the holes should be reamed to final rivet diameter by a *fluted* reamer—not the common half-round reamer in general use in a majority of boiler shops. With rivet holes properly spaced, punched or drilled, and reamed as described above, with the outside and inside of the hole slightly countersunk by a *die*—not cut away—the value of the joint may be safely

trusted at sixty-five per cent. of the plate value for single and eighty per cent. for double-riveted joints.

The fact that the greatest strength of any structure is only equal to that of its weakest part is equally true of steam boilers.

I will, therefore, give my practice in securing the strongest practical joint. The proper proportion of the seams of boiler shells to obtain the greatest strength is the aim of all engineers.

In the majority of boilers commercially made the rivet-section area exceeds that of the plate-section area as much as *forty per cent.* This should not be so.

It is well known that the majority of plates used for boilers is weakened by the amount of material punched out, and that that which is left after punching is very much deteriorated, around the holes, if inferior plates are used, by the strain to which the metal has been subjected, on account of the large clearance allowed by a majority of boiler-makers between the diameter of the punch and that of the bolster or die.

The effects of the strains to which plates are subjected, due to the large clearance allowed, as well as from the use of inferior boiler plates, can be seen by examining any ordinary punched plate rivet hole. The material always has a peculiar granular appearance, showing that the fibre has been destroyed; and by taking a piece of boiler plate which has been punched, planing it through the centre of the punched holes and rivets, and then polishing the section and applying dilute muriatic acid, the effect of the punch upon the fibres of the iron can distinctly be seen.

In addition to holding boiler plates together, rivets have another function to perform, which is to keep the joints water- and steam-tight. To do this they must be placed near enough to each other to hold the plates together, thus preventing them from leaking between the rivets. If the rivets do not fill the holes, there is also danger of their leaking around the heads. Now, a badly fitted or crooked rivet, obviously, will not hold the plates together so well as one which is perfectly straight and fits the hole accurately. Any one who has ever observed two plates which have been



punched rivet size (by this I mean the size the rivet becomes after being driven, the usual practice being to punch the holes one-sixteenth or 0.0625 of an inch greater in diameter than the diameter of the rivet to be used), and placed together, knows it is impossible to make the holes correspond with any degree of accuracy, and that they must either be strained into position by driving a drift-pin into the holes or else reamed out to get the rivet to enter. The manner in which rivets are practically fitted will be seen from the following, *Figs. 1 and 2*, made from pieces of old boilers, planed through the centre line of the rivets.

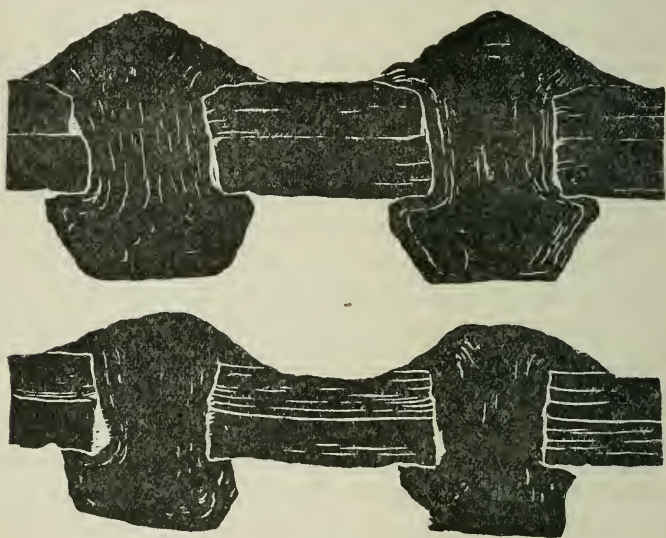


FIG. 1.

From these sections impressions with ordinary printer's ink were transferred to blocks and engraved, therefore, they may be relied upon as entirely accurate. They are by no means examples of the worst specimens, as they were selected at random from old boilers. The engraver has endeavored to reproduce the appearance of the fibre of the iron caused by the punch passing through it. It will be seen that the metal for a space of from one to three-sixteenths of an inch around the hole has been "fatigued," or strained, so that apparently little of its strength is left. Of

course, where iron is subjected to so great a strain, besides injuring its fibre, there is danger of cracking it and thus creating a flaw. It will also be seen that the holes do not correspond with each other, and that, therefore, some of the rivets are crooked and some do not fill the holes.

It will also be seen by *Figs. 1 and 2* that the rivets are fitted so badly into the holes that they would evidently resist very little shearing strain, which must be taken by the adjoining rivets. It is true that when plates are held together there will be considerable friction between them, but this friction may not act simultaneously with the

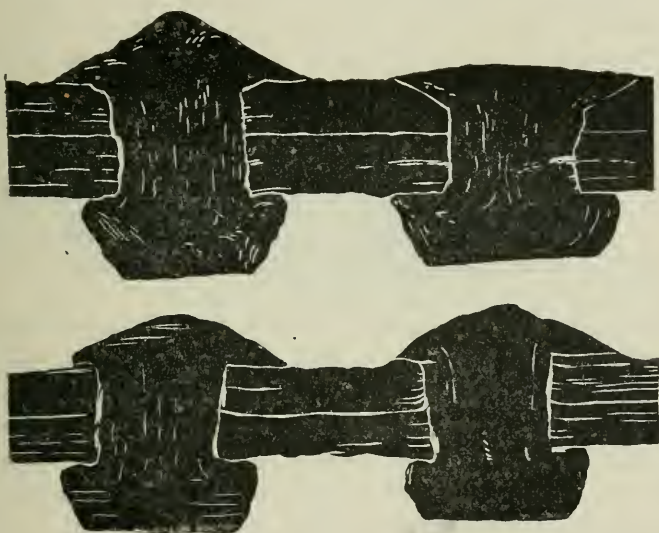


FIG. 2.

resistance to shearing, owing to irregularity of surfaces, so that the force acting upon the boiler plates might be resisted first by the rivets and then by the friction of the plates upon each other. The total strength of the plates is, of course, less, separately, than when taken together. Now, if the holes were fair and the rivets driven into the holes properly, the strain on the seam would be resisted equally by all of the rivets conjointly, instead of by a portion only, and also by the maximum friction of the plates.

To get the best result, I have, in my practice, specified in

boiler contracts that all rivet holes shall be punched the size of the rivet to be used and that when the plates are in their respective positions, held by temporary bolts, the holes shall be reamed one-sixteenth of an inch diameter with a fluted reamer, after which the inside and outside of shell plate shall be countersunk by a steel die (not cut away by a sharp-edged tool).

By the above process the annular ring around the rivet hole, due to detrusion of the punch, is removed, the metal which is left between the holes of the plate is uninjured, and consequently is much stronger than would be the case if the holes had been punched or drilled full rivet size. The countersinking of the rivet holes, as shown in *Fig. 3*, will be filled up by the rivets and thus add materially to the strength of the latter at their weakest point.



FIG. 3.

By punching the holes of a smaller diameter than the driven rivet size, and afterwards reaming the same in place, the minimum reduction in area of the plate by punching will be attained, and consequently the maximum strength of the complete structure; and as the rivets will be much better fitted, they can be spaced farther apart, thus leaving more metal in the plates, while at the same time the latter will be held together equally well.

With the holes punched small and reamed to full rivet size, they will always conform exactly with each other, even with careless workmanship, so that the danger to which we are always liable with boilers whose rivet holes have been punched or drilled—that of having undue strain fall upon some of the rivets—will be obviated, and at the same time the metal left between the holes will have more strength than when punched or drilled in the usual way; the danger of leakage around the rivets will be obviated to a great extent and, therefore, they can also be spaced farther apart.

By increasing the diameter of the rivets, their transverse sectional area is increased much more rapidly than the longitudinal section of the holes they must fill, so that giving them sufficient strength does not involve taking away an equal sectional area of the plates.

Besides the weakening of the plate by the strain to which it is subjected by careless punching, there is always a degree of uncertainty about the strength of seams if the plates are punched to full rivet size, which must be a fruitful cause of weakness and consequent disaster. Every engineer, boiler-maker or person who has made even the most casual observation in a boiler shop, knows that as the holes are laid out and punched separately to full rivet size, they never *correspond exactly*, however great the care taken in spacing. Frequently they all come obliquely and must either be reamed out for a larger rivet, or else the

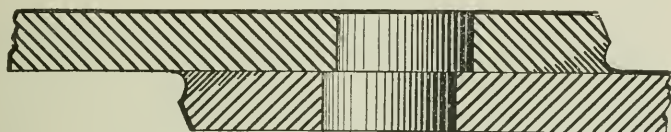


FIG. 4.

plates are strained by driving in a drift-pin to force them in position, in order to bring the holes near enough over each other, to allow the rivet to enter. That they do not fill the holes when the plates are in such a position is distinctly shown if a seam be planed through the centre line of the rivets, as shown in *Figs. 1 and 2*.

Now, suppose that some of the holes in the two plates come in relation to each other, as they often do, in the manner represented in *Fig. 4*. In such an event it will be almost impossible to make the rivet fill the holes unless the latter are reamed out much larger than the size they are punched. If this is done a larger sized rivet must be used, which can be made to fit accurately, but entire dependence must be placed on the faithfulness of the workman employed to do this—a reliance, the uncertainty of which only those who have had experience know, and which, I regret to say, of late years seems to be growing more and more insecure.



Now, the fact that workmen *could* do good work if they would, does not lessen the danger from this source. In making boilers with rivet holes punched to full rivet size there is always a risk of bad workmanship, and, what is worse, it is always out of sight and therefore undiscoverable.

With boiler plates carelessly punched to full rivet size there is also danger that some of the rivets will be subjected to an undue strain. If, for example, we have three rivets, as shown in *Fig. 5*, of which only the centre holes correspond with each other, and consequently are the only ones which are fitted accurately by a rivet, all the others will fill the holes probably somewhat, as in *Figs. 1* and *2*, or as shown in *Fig. 6*. Now, if the middle rivet in *Fig. 5* should fit accurately, while

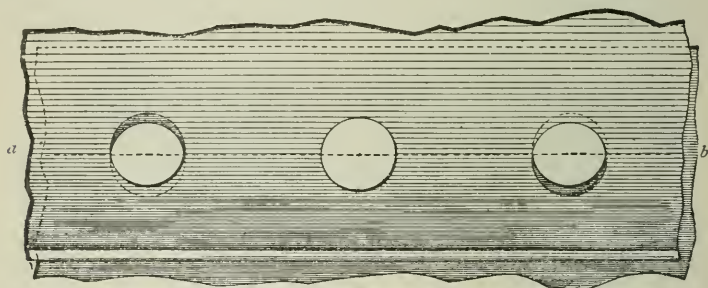


FIG. 5.

all the others should fit, as shown in *Fig. 6* (which probably frequently does occur), then the centre rivet must bear all the strain until the others are drawn up to a bearing. There would, of course, under such circumstances, be very great danger of shearing off the rivet or, of starting a fracture in the sheet, as indicated by the dotted line *a b*, in *Fig. 5*. When once started the constant expansion and contraction from the strain in the boiler would, of course, have a tendency to increase it, until there would not be sufficient strength left to resist the steam pressure, and a "mysterious" (?) explosion would result.

Therefore, I claim that just so long as boiler plates are punched or drilled full rivet size there will be difficulty in getting the holes to correspond exactly with each other, and danger from bad workmanship will exist.



This difficulty and danger can be entirely overcome by using the best material and punching the rivet holes one-sixteenth of an inch less than full rivet size, with an excess of plate area over that of the rivet, and, when the parts are in place, reaming the rivet holes to full rivet size; those portions of the plate left between the holes will then be from *ten to twenty per cent.* stronger than the commercially made boilers at the present time.

The additional cost of making boiler joints in the manner above described, is insignificant as compared with the practice in vogue, and in view of the great advantages gained. To illustrate, the additional cost of an eighty horse-power boiler constructed by one of our leading boiler-makers, in the manner contended for, was only fifteen (15) dollars.

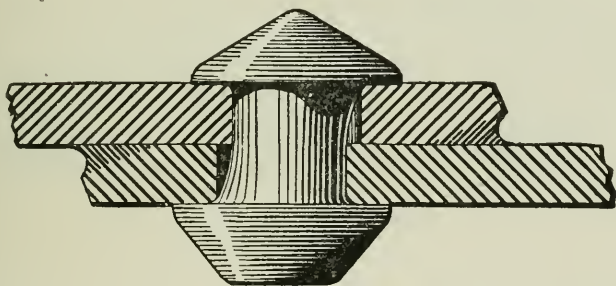


FIG. 6.

When we take into consideration the advantages gained by the buyer in having a better-made structure, which will be capable of doing more work by the increased steam pressure, at which it will be practicable to run it, the increase in cost will be a good investment.

From the foregoing, it will be seen that a single riveted joint, made as described, will be twenty per cent. stronger than the ordinary riveted plates having the rivet holes punched or drilled full rivet size. As has been shown, this difference in strength is attributable partly to the injury done to the plates by punching and to the presence of the injured parts. When the holes are punched of a smaller diameter than full rivet size, and afterward reamed to the latter dimensions, the reaming removes all the portions

injured by the punch. Furthermore, all the rivet holes coincide exactly with each other, and, therefore, the rivets will fit perfectly and will also be less likely to leak. For this reason the rivets can be spaced farther apart, and, by increasing their diameter slightly, their sectional area will be sufficiently great to equal that of the plate metal left between the holes, as shown in *Fig. 7*, composed of three-eighths-inch plates.

The objection raised, as before stated, to spacing the rivets for three-eighths-inch plates, as shown in *Fig. 7*, is that the seams are liable to leak between the rivet holes. In other words, the plates will not be held together tight enough to prevent the forcing of steam and water between them. The greatest distance that rivets may be spaced

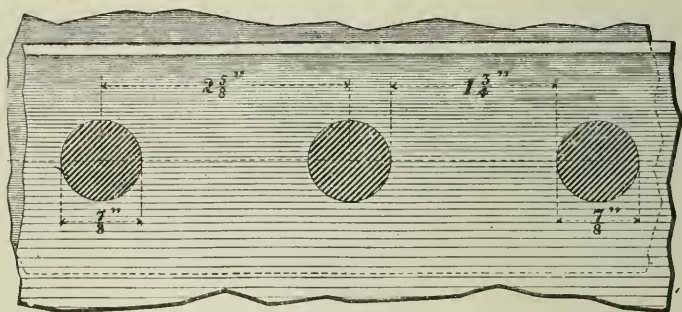


FIG. 7.

apart without incurring danger of leakage between the plates must, however, be determined more by practical than theoretical considerations. What I wish to make clear is, that a boiler joint will be stronger if the diameter and distance apart of the rivets are increased in the proper proportion to each other.

There is, however, a limit beyond which the diameter of rivets cannot be increased with advantage, because if we increase their diameters, their sectional area to resist shearing is increased in proportion to the square of the diameter, whereas the section of metal in the plate to resist crushing is increased only as the distance between the holes.

A given increase to the diameter of the rivets will therefore increase their sectional area much more than the

material of the plates will be cut away by enlarging the rivet holes.

This will be apparent if we compare a rivet one-half inch in diameter with one one inch in diameter. The first has a sectional area of 0.1963 square inch, the other 0.7854 square inch, or *four times* that of the first one. Now the area which resists the crushing strain of the rivets is increased only in proportion to their diameters, or *twice* as much for the one as for the other. If, therefore, we increase the diameters of the rivets, we very soon reach a point at which the plate has less strength to resist crushing than the rivet has to resist shearing. The diameter of a rivet which will give just the same resistance to both strains varies with the thickness of the plates. With three-eighths-inch thick plates a seven-eighths-inch diameter rivet will have a resistance to shearing of about 30,000 pounds, and the three-eighths-inch plates in front of it, a resistance to crushing also of about 30,000 pounds. A seven-eighths-inch diameter rivet is, therefore, the largest sized iron rivet which can be used to advantage in three-eighths-inch plates.

If now we were to space such rivets so far apart that the metal left between the rivet holes would equal the area of the rivet in cross section, we would have a strength just equal to that of the rivet; we would have the strongest possible seam or joint that can be made with a single row of rivets.

In plates three-eighths-inch thick, and seven-eighths-inch diameter, rivets of the above value being employed, the distance between the margins of the rivet holes would be one and three-fourths inches, or two and five-eighths inches from centre to centre of rivet holes. (See *Fig. 7.*)

I have been explicit on this point, because the distance of the rivets apart has an important bearing where double-riveted joints are used. The advantages of double-riveted joints over single-riveted joints are that the strain is distributed among a greater number of rivets, and therefore the rivets may be smaller, and consequently less material cut out of the cross section of the plates for the rivet holes.

All engineers are aware of the great advantage gained

in double-riveted joints by placing the rivet holes in two rows alternately to each other, or in spacing them "zigzag" or "staggered," as shown in *Fig. 8*, which represents three-eighths-inch thick plates, which is the thickness in most general use for steam-boiler purposes.

It has been found in practice that for the greatest strength, the distance  $K$  between the centre of rivet holes  $c c$ , *Fig. 8*, or in the direction of the joint, should be double the distance  $\frac{1}{2}K$  between the centre lines of rivet holes of the two rows, and the rivets will then form a right angle or  $90^\circ$  with one another. The distance between the rivet holes in the direction of  $a c$ , or the joint, can be made fifty

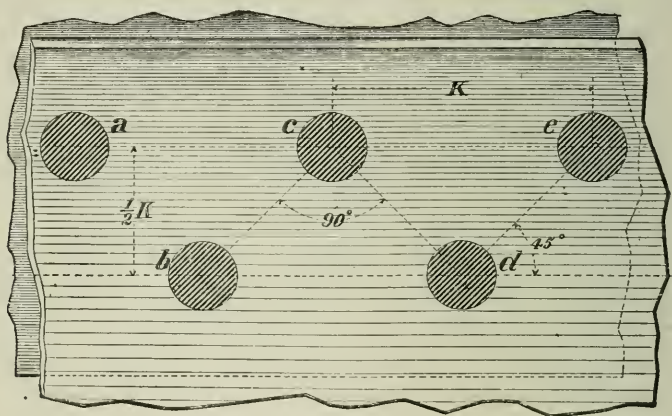


FIG. 8.

per cent. greater than between rivet holes in single-riveted joints. The diagonal distance between centres of rivet holes should be made equal to the distance in the direction of the joint in single riveting.

In regard to spacing rivet holes zigzag, as shown in *Fig. 9*, it will be seen that the rivets in the respective rows are much nearer together than those in the same row, but the sectional area of plate in the line  $a b c d$  and  $e$  is over two square inches, whereas, in the line  $f g$  it is only one and nine-tenths square inches. Therefore, although the rivets on the zigzag line are nearer together than those in the same row, yet the line  $a b c d$  and  $e$  presents more material to resist rupture. The strongest joint of this kind



will be one in which the material between the rivet holes—say  $b d$ —in the same line, is equal to two of the spaces  $a b$  and  $b c$ , between the rivets in adjoining lines. Or, to express it algebraically, and supposing  $x$  to represent the distance between the circumference of the rivet holes  $b$  and  $d$  and  $m$ , and  $n$  equal to that between  $a$  and  $b$ , and  $b$  and  $c$ , then we ought to have:

$$x = m n$$

In order to make the strongest double-riveted joint of this kind, we must, therefore, first determine upon the greatest distance which is admissible between the rivet holes in order to keep the boiler joint steam- and water-tight. Assuming this distance to be one and three-eighths

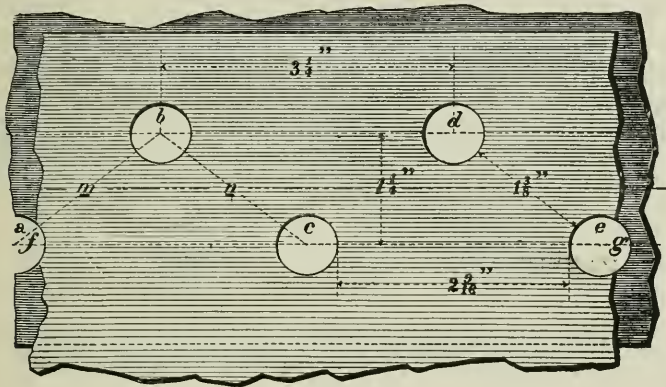


FIG. 9.

inches, as shown in *Fig. 9*, then the spaces between the rivet holes on the *same line* should be two and three-quarters inches. If the plates are punched less than full rivet size, and when in place reamed to full rivet diameter, the sectional area of the rivets should be equal to that of the plates between the rivet holes. (This is assuming that the tensile strength of plates and rivets are in value equal to each other, or nearly so.) As there are two rivets to resist the strain due to the space between the adjoining rivets, as  $b d$ , for example, therefore, the sectional area of each rivet should be just half that of the plate section between the rivet holes  $b d$ . With a three-eighths-inch thick plate, this latter area would be one and three-hundredths square inches,



the half of which would correspond very nearly with the area of a thirteen-sixteenths-inch rivet.

Having the distance between the rivet holes on the same line and the diameter of the rivets, it is, of course, easy to establish their pitch, which in this case, would be three and nine-sixteenths inches from centre to centre as represented in *Fig. 10*.

It only now remains to establish the position of the rivet hole *c* in the adjoining line. This can easily be done by taking a radius, *ae* or *bd* equal to one-half the distance between the centres of the rivet holes *a* and *b*, and from them describing two arcs *pe* and *pd*. If, now, the rivet hole *c* be located so as to be tangent to these two arcs, then the

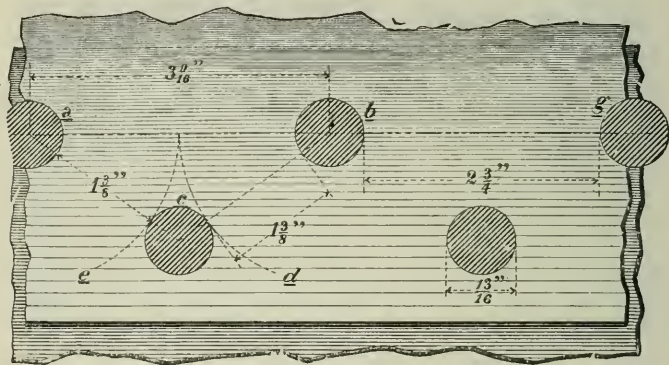


FIG. 10.

spaces between the rivet holes *ac* and *cb*, will be just equal to that between *a* and *b*. The plates will, therefore, have the same strength in the line *acb* as in *ab*. The centre of the rivet hole *c* can be located, perhaps, more easily, by taking half the pitch *ab*, plus half the diameter of the rivet hole as a radius, and describing from *a* and *b* two arcs intersecting at *c*, which will be the centre of the rivet hole.

In the ordinary course of boiler-making almost every error of workmanship tends to weaken the plates and give an excess of rivet area, while, in addition, the corrosion, which to a greater or less extent takes place during the life of a boiler, also weakens the plate while scarcely affecting the rivets.

It is for these reasons that I consider that a riveted boiler joint should be laid out so as to have an excess of plate section over that of the rivet.

It is impracticable to proportion the riveted joint so perfectly that the shearing strength of the rivet will be equal to the tearing strength of the plate, for the actual strength of the plates varies more than does the proportion of dimensions of the joint.

[To be continued.]

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## ANALYTICAL DISCUSSION OF THE TIDAL VOLUME

ADMITTED INTO BAYS AND RIVERS UNDER GIVEN CONDITIONS OF WIDTH, DEPTH AND AREA OF BASIN—RELATION BETWEEN WIDTH AND DEPTH IN THE CROSS SECTIONS OF TIDAL STREAMS WHOSE BEDS ARE YIELDING TO THE ACTION OF CURRENTS—VARIATION OF CROSS-SECTIONAL AREA IN SUCH STREAMS.

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BY L. D'AURIA.

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If a perfect freedom of flow existed in a tidal river of uniform width and uniform depth, the range of tide in such river would also be uniform, and the profile of the tidal wave could be identified with the curve of sines.

Imagine any portion of this ideal tidal river, from its head down to any given cross section, and let us find the analytical expression of the tidal volume which would flow through such cross section, from slack water to slack water.

In the first place we will observe that slack water in any given cross section of the tidal river under consideration would occur when either the crest or the trough of the tidal wave occupies a position midway between the given cross section and the head of the river. When the crest is in such position, we would have what is generally called *high-water slack*, and when it is the trough, we would have the *low-water slack*, through the cross section.



tions, though theoretical, represent facts generally observed in tidal rivers in a greater or less degree.

Let  $MCN$  and  $M'C'N'$  (*Fig. 1*), represent the profiles of the tidal wave in the two positions which correspond to high-water slack and low-water slack, respectively, through the cross section  $MM'$ , whose distance from the head of the river  $NN'$  is  $z$ ; and let  $\lambda$  denote the length of the wave;  $R$  the height from crest to trough, and  $b$  the width of the river. It is obvious that the tidal volume passing through the cross section  $MM'$  from one slack water to the other will be

$$q = b (R z - 4 \times \text{area } C'E'N')$$

and the problem is reduced to find the area  $C'E'N'$ .

Let  $x$  and  $y$  represent the coördinates of any point of the curve  $C'N'$  referred to  $C'$  as origin. Then will be found

$$y = R \sin^2 \left( \frac{\pi x}{\lambda} \right) \quad (1)$$

and

$$\text{area } C'E'N' = R \int_0^z \sin^2 \left( \frac{\pi x}{\lambda} \right) dx$$

Hence,

$$q = b R \left( z - 4 \int_0^z \sin^2 \left( \frac{\pi x}{\lambda} \right) dx \right)$$

Integrating between the assigned limits we find, after reduction,

$$q = \frac{\lambda b R}{\pi} \sin \left( \frac{\pi z}{\lambda} \right) \quad (2)$$

Let  $T$  represent the mean tidal interval in seconds or  $T = 44,600$  seconds, approximately. Then if  $v$  denotes the mean velocity of wave propagation in the distance  $z$  we can put  $\lambda = Tv$ , and write.

$$q = \frac{T v b R}{\pi} \sin \left( \frac{\pi z}{Tv} \right) \quad (3)$$

This expression becomes a maximum when  $z = \frac{1}{2} v T$ , which would show that the greatest tidal volume which can

be admitted into a tidal basin whose width is  $b$  and whose length exceeds the quantity  $\frac{1}{2} v T$  is

$$Q = \frac{T v b R}{\pi} \quad (4)$$

since it would be impossible from a physical point of view to admit that by increasing the length of the tidal basin beyond the limit  $\frac{1}{2} v T$ , a diminution of tidal volume would ensue. Hence, from such standpoint equation (4) ought to hold good for all cases where  $s = > \frac{1}{2} v T$ , while equation (3) ought to be applied to cases where  $s < \frac{1}{2} v T$ .

Now it may be reasonably assumed that two tidal basins of equal area but different shapes, having the same width of entrance, the same average depth and the same range of tide at the entrance would admit an equal tidal volume during each tide. With this assumption, if we denote the area of the basin by  $A$ , we can put  $s = A \div b$  in equation (3), which then becomes

$$q = \frac{T v b R}{\pi} \sin \left( \frac{\pi A}{T v b} \right) \quad (5)$$

with the limitation of  $A < \frac{1}{2} v T b$ ; and when  $A = > \frac{1}{2} v T b$ , we have to apply equation (4).

In cases where the widths of the cross sections change from low to high water, the area  $A$  of the tidal basin will have to be computed at low water, and the width  $b$  at mid-tide. In order to appreciate the correctness of this remark, imagine first a tidal basin of uniform width from low to high water. Now, if without altering the cross section of the entrance to such basin we were to increase gradually the widths of all the other cross sections from low to high water without touching the low-water widths, it can hardly be expected that by such operation the tidal volume would be increased, although a considerable increase in the mean area of the basin has taken place. Such increase would evidently cause a reduction of tidal range in the upper cross sections, and this would compensate for the increased mean area of the basin, as far as the tidal volume is concerned.

If we denote by  $\beta$  the mean low water width of the tidal basin, by  $l$  its length, and by  $\theta$  the average of the time



intervals of high water and low water in the distance  $l$ , we can put equation (5) under the form

$$q = \frac{T l b R}{\pi \theta} \sin \left( \frac{\theta \beta \pi}{T b} \right) \quad (6)$$

In order to test this formula, let us apply it to a well-known case where the necessary data have been well determined by observations. This case we find in the portion of the Delaware River, between Bridesburg (above Philadelphia) and Trenton, for which we have  $l = 144,000$  feet;  $b = 3,860$  feet;  $\beta = 1,764$  feet;  $R = 5.98$ , and  $\theta = 162$  minutes. Putting  $T = 762$  minutes, we obtain by substituting in formula (6)

$$q = 1,496,000,000 \text{ cubic feet.}$$

Now, according to an extremely accurate measurement of the tidal volume in question deduced from simultaneous observations, made by the United States Engineers, it has been found 1,452,000,000 cubic feet, which is only about three per cent. less than the theoretical.

It may be that taking into account the volume of fresh water discharged by the river, this difference would be somewhat larger, but we only claim to give the theoretical, not the actual tidal volume.

When the basin is shaped like a bay, with narrow entrance, it is rather difficult to obtain the proper values of  $\theta$  and  $l$ , which are required by formula (6), to compute the tidal volume. However, if we knew the value of  $v$  in equation (5), we could use this equation instead of the (6) in computing the tidal volume, independently of  $\theta$  and  $l$ . Now, if we denote by  $(H_0)$  the average depth of the basin at mid-tide, we can put approximately

$$v = 4.81 \sqrt{(H_0)}$$

[See "A new theory of the propagation of waves in liquids. By the author. *Journal Franklin Institute*, December, 1890.]

Then we can write:

$$q = \frac{4.81 b T R \sqrt{(H_0)}}{\pi} \sin \left( \frac{\pi A}{4.81 b T \sqrt{(H_0)}} \right) \quad (7)$$

When

$$A = > \frac{1}{2} 4.81 b T \sqrt{(H_0)}$$

which is the case of very large bays with narrow entrances, then, instead of equation (4), we can use

$$Q = \frac{4.81 b T R \sqrt{(H_0)}}{\pi} \quad (8)$$

In order to test formula (7) we have taken the case of Absecom and Brigantine Bay, Atlantic City, N. J., for which we have the following data:  $A = 362,400,000$  sq. ft.;  $b = 1760$  ft.;  $(H_0) = 9.5$  ft.;  $R = 4$  ft. Substituting these data in equation (7) we find

$$q = 1,482,000,000 \times \sin 56^\circ$$

or

$$q = 1,228,000,000 \text{ cubic feet.}$$

By gauging, and with the same range of tide of four feet, Mr. George Daubeney, Assistant Engineer, found in 1880 a tidal volume equal to 1,174,612,000 cubic feet [see *Report of Chief Engineers U. S. Army*, 1881]; and in 1886, by simultaneous tidal observations with the same range of tide was found again, by the writer a tidal volume equal to 1,170,614,000 cubic feet. [See *Report of Chief Engineers U. S. Army*, 1887.]

Both these measurements, surprisingly near to each other, are in the mean only four per cent. less than the theoretical tidal volume computed by formula (7). Such close agreement, we believe, entitle us to view our formulæ from a physical point and trust the conclusions which may thus be reached. Now we may readily see from formula (8) that in very large tidal basins with narrow entrances the tidal volume is directly proportional to the width of entrance, showing that in large tidal bays it would be useless to change the width of entrance with a view to increase the velocity of the currents through such entrance; and as far as the upper navigation of such bays is concerned, it teaches that by narrowing the width of entrance we cut away from the various channels an amount of flow proportionately to the reduction of width of entrance effected.

In tidal rivers, however, where formula (7) is applicable, this formula would show that the tidal volume varies with the quantity

$$\phi = b \sin \left( \frac{\pi A}{4.81 b T_1 \sqrt{H_0}} \right) \quad (9)$$

other things remaining the same. Now, it can be seen that this quantity is only feebly affected by  $b$  when the area of the basin  $A$  is small compared with

$$2.4 b T_1 \sqrt{H_0}$$

In fact, suppose for instance that we should find

$$\frac{A}{2.4 b T_1 \sqrt{H_0}} = \frac{1}{3}$$

then the tidal volume would be proportional to  $\frac{1}{2} b$ . If instead of  $b$  we substitute, say  $\frac{2}{3} b$  in (9), then the tidal volume would become proportional to  $0.47 b$ , showing, therefore, a loss of only six per cent. of tidal volume against a reduction of thirty-three per cent. of the width of entrance.

Let  $\phi_1$  represent the value of  $\phi$  corresponding to the width  $b_1 < b$ ; then the loss of tidal volume incurred would be expressed by

$$j = \left( 1 - \frac{\phi_1}{\phi} \right) q$$

In the above argument we have tacitly supposed that the reduction of width amounts to a contraction of the river at a particular point, forming a kind of bay above it; but where only a rectification of the banks of the river is contemplated no loss of tidal volume can possibly follow.

In projecting improvements of tidal rivers it has always been a perplexing question how far the tidal volume might be affected. We can answer this question now, fully, by consulting our formulæ (9) and (10), when the necessary data are given; and in a general way we have learned that where the ratio

$$A \div 2.4 b T_1 \sqrt{H_0}$$

is considerably smaller than *one*, say  $\frac{1}{4}$  or  $\frac{1}{3}$ , which would be the case of the upper sections of tidal rivers, then a contraction of width may be effected, practically without loss of

tidal volume, a conclusion which may save a great amount of discussion relating to the improvement of such sections of tidal rivers.

In tidal rivers whose beds are yielding to the action of currents the depth of the cross sections must bear some relation to the mean velocity of the stream. Let  $u$  represent this velocity through a cross section  $\Omega$  whose depth is  $H$ , and put  $H = cu^n$ , in which  $u$  is to be determined, and  $c$  is a constant. Now, from formula (7), we can compute

$$u = \frac{4.81 R^{1/3} (H_0)^{1/3}}{\pi H} \sin \left( \frac{\pi A}{4.81 b T^{1/3} (H_0)^{1/3}} \right) \quad (10)$$

and consequently we find

$$H = c_1 R^{\frac{n}{n+1}} (H_0)^{\frac{n}{2(n+1)}} \sin^{\frac{n}{n+1}} \left( \frac{\pi A}{4.81 b T^{1/3} (H_0)^{1/3}} \right) \quad (11)$$

This formula when applied to the Delaware River led the writer to the conclusion  $n = 2$ , hence

$$H = c_1 R^{\frac{2}{3}} (H_0)^{\frac{1}{3}} \sin^{\frac{2}{3}} \left( \frac{\pi A}{4.81 b T^{1/3} (H_0)^{1/3}} \right) \quad (12)$$

Changing the width  $b$  of the cross section  $\Omega$ , this formula offers the corresponding mean depth when other things are given. Let, for instance,  $b_1$  and  $H_1$  represent the new width and the new depth, we would have

$$\frac{H_1}{H} = \left( \frac{R_1}{R} \right)^{\frac{2}{3}} \cdot \frac{\sin^{\frac{2}{3}} \left( \frac{\pi A}{4.81 b_1 T^{1/3} (H_0)^{1/3}} \right)}{\sin^{\frac{2}{3}} \left( \frac{\pi A}{4.81 b T^{1/3} (H_0)^{1/3}} \right)} \quad (13)$$

When  $A$  is small compared with

$$2.4 b T^{1/3} (H_0)^{1/3}$$

and the ratio does not exceed, say  $\frac{1}{2}$ , we can write simply

$$\frac{H_1}{H} = \left( \frac{R_1}{R} \right)^{\frac{2}{3}} \left( \frac{b}{b_1} \right)^{\frac{2}{3}} \quad (14)$$

If we multiply equation (12) by  $b$  we have

$$\Omega = c_1 b R^{\frac{2}{3}} (H_0)^{\frac{1}{3}} \sin^{\frac{2}{3}} \left( \frac{\pi A}{4.81 b T^{1/3} (H_0)^{1/3}} \right) \quad (15)$$

or, remembering that we have expressed the velocity of propagation by

$$v = 4.81 \sqrt{H_0}$$

and that  $A = s\beta$  in which  $s$  is the length and  $\beta$  the average width of the basin, we can readily find

$$Q = c_2 b R^{\frac{2}{3}} \left(\frac{s}{\theta}\right)^{\frac{2}{3}} \sin^{\frac{2}{3}} \left(\frac{\beta \theta \pi}{b T}\right) \quad (16)$$

For the upper portion of a tidal river we find  $\theta$  considerably smaller than  $T$ , and therefore, for such case, we can assume the sine proportional to

$$\left(\frac{\beta \theta}{b}\right)$$

and write

$$Q = c_3 R^{\frac{2}{3}} s^{\frac{2}{3}} \beta^{\frac{2}{3}} b^{\frac{1}{3}} \quad (17)$$

and

$$\frac{Q_1}{Q} = \left(\frac{R_1 \beta_1}{R \beta}\right)^{\frac{2}{3}} \left(\frac{s_1}{s}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} \quad (18)$$

When the cross sections are not too far apart we can consider the mean width of the basin and the range of tide to remain constant, and then we have

$$\frac{Q_1}{Q} = \left(\frac{s_1}{s}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} \quad (19)$$

In order to test this relation we have selected three pairs of cross sections from the Delaware River, each pair exhibiting a great variation of width in a short distance, and the results are as follows:

Fisher's Point:

$$Q = 52,927 \text{ sq. ft.}; b = 2,610 \text{ ft.}; s = 144,000 \text{ ft.}$$

Elevator Wharf:

$$Q_1 = 67,380 \text{ sq. ft.}; b_1 = 4,240 \text{ ft.}; s_1 = 154,000 \text{ ft.}$$

$$\frac{Q_1}{Q} = 1.27; \left(\frac{s_1}{s}\right)^{\frac{2}{3}} \left(\frac{b_1}{b}\right)^{\frac{1}{3}} = 1.23.$$

Gloucester:

$$Q = 53,625 \text{ sq. ft.}; b = 1,800 \text{ ft.}; s = 186,000 \text{ ft.}$$



Horse Shoe :

$$Q_1 = 91,380 \text{ sq. ft.}; b_1 = 5,850 \text{ ft.}; s_1 = 196,000 \text{ ft.}$$

$$\frac{Q_1}{Q} = 1.70; \left( \frac{s_1}{s} \right)^{\frac{2}{3}} \left( \frac{b_1}{b} \right)^{\frac{1}{3}} = 1.53.$$

Deep Water Point :

$$Q = 140,500 \text{ sq. ft.}; b = 4,860 \text{ ft.}; s = 326,000 \text{ ft.}$$

New Castle :

$$Q_1 = 182,200 \text{ sq. ft.}; b_1 = 8,400 \text{ ft.}; s_1 = 344,000 \text{ ft.}$$

$$\frac{Q_1}{Q} = 1.30; \left( \frac{s_1}{s} \right)^{\frac{2}{3}} \left( \frac{b_1}{b} \right)^{\frac{1}{3}} = 1.24.$$

On examination it will be found that at Gloucester the bottom is rather hard compared with the other cross section, a circumstance which seems to account for the difference of ten per cent. found in the second case between the actual and the theoretical result. The small difference found in the other two cases, though utterly insignificant in computations of this kind, still is in a direction which is in accordance with the factor neglected in formula (18). Had this factor been computed a still closer agreement would have been found between the results of observation and those obtained by theory.

## ON A MAXIMUM STEAM-JACKET EFFICIENCY.

BY ROBERT H. THURSTON.

(1) IDEAL EFFICIENCY.—The fact is sufficiently well known that the steam jacket, as employed on the steam engine, of whatever form and arrangement, is intrinsically a wasteful element, and that its use only gives, in certain cases, an economical advantage by its repression of wastes of larger magnitude. It checks a serious and unavoidable waste, more or less completely, by a process which as inevitably involves a waste which is commonly, but, perhaps, not invariably, a lesser one. The ideal steam engine, such as is treated of in the purely thermodynamic study of the steam engine, has a lower efficiency with, than it has with-

out, a jacket. This is readily seen from the following illustrations computed and checked by Messrs. Hitchcock and Mount, at the suggestion of the writer. Nine cases are examined, in which the same initial pressure is taken and the efficiencies are computed both with and without jacket for various ratios of expansion. The results, as given in the following tables and as illustrated in the curves plotted from them, show that the jacketed engine is always more wasteful than the ideal, unjacketed engine. This is sufficiently evident, *à priori*, from the consideration that the latter receives all its steam at a maximum temperature, expands it adiabatically to a certain terminal temperature, and then exhausts it; while the former receives a part of its heat at intermediate temperatures, expands the fluid non-adiabatically, and finally rejects it at the terminal temperature, with a lower mean range of expansion. In other words, the jacketed engine departs furthest from the principles of economical operation first enunciated by Carnot: \* All heat should be received at maximum temperature; expansion should be perfectly adiabatic, and should continue to the minimum temperature and pressure, and all should be rejected as nearly as possible at that minimum. Thus, "theoretically," if the use of that much-abused term may be permitted in this sense, the unjacketed engine is more efficient than the jacketed engine.

"Practically," however, the reverse is usually, though probably not always, the case, and the use of the jacket is often found to be productive of a real, and sometimes of large, economy. It is thus obvious that the advantages of the employment of the jacket come of those conditions which distinguish so markedly the real from the ideal case in steam-engine economy: those which make the "Theory of the Real Engine," as the writer has called it, essentially different, in important respects, from the "Theory of the Ideal Engine." †

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\* *Reflections on the Motive-Power of Heat* By Sadi Carnot. Edited by R. H. Thurston. New York: J. Wiley & Sons. 1890.

† *Development of the Philosophy of the Steam Engine.* Thurston, N. Y.: J. Wiley & Sons. 1889.

The following illustrates this comparison :

EFFICIENCY OF STEAM—IDEAL CASES. RANKINE.

*Non-conducting cylinder.*

- $p_1$  = absolute pressure of admission ;  
 $p_2$  = absolute pressure at end of expansion ;  
 $p_3$  = mean back absolute pressure ;  
 $\tau_1$  = absolute temperature of  $p_1$  ;  
 $\tau_2$  = absolute temperature of  $p_2$  ;  
 $\tau_4$  = absolute temperature of feed water.

Then the energy per cubic foot of steam admitted :

$$U D_1 = J D_1 \left\{ \tau_1 - \tau_2 \left( 1 + \text{hyp log } \frac{\tau_1}{\tau_2} \right) \right\} + \frac{\tau_1 - \tau_2}{\tau_1} L_1 - r (p_2 - p_3)$$

- $r$  = ratio of expansion ;  
 $D_1$  = density of steam at  $p_1$  ;  
 $L_1$  = latent heat of steam at  $p_1$ .

Heat expended per cubic foot of steam admitted :

$$H_1 D_1 = J D_1 (\tau_1 - \tau_4) + L_1 ;$$

$H_1$  = latent heat of evaporation ;

$$\text{Eff. of steam} = \frac{U}{H_1}$$

*Cylinder Jacketed.*

- $p_1$  = absolute pressure of admission ;  
 $p_2$  = absolute pressure at end of expansion ;  
 $p_3$  = mean absolute back pressure ;  
 $\tau_1$  = absolute temperature of  $p_1$  ;  
 $\tau_2$  = absolute temperature of  $p_2$  ;  
 $V_1$  = volume of 1 lb. steam at  $p_1$  ;  
 $V_2$  = volume of 1 lb. steam at  $p_2$  ;  
 $a$  = 1109550 ft. lbs. ;  
 $b$  = 540.4 ft. lbs. per degree F.

Then energy exerted :

$$U^1 = a \text{ hyp log } \frac{\tau_1}{\tau_2} - b (\tau_1 - \tau_2) + V_2 (p_2 - p_3)$$

Whole expenditure of heat per lb. steam:

$$\dot{h} = J (\tau_2 - \tau_4) + a \left( 1 + \text{hyp log } \frac{\tau_1}{\tau_2} \right) - b \tau_1$$

Eff. of steam:

$$\text{Eff.} = \frac{U^1}{h}$$

#### COMPUTATIONS—IDEAL CASE.

##### *Without Jacket.*

*Case 1.*

$$\begin{aligned} p_1 &= 115 \text{ lb. at.} & \tau_1 &= 799 & r &= 20 \\ p_2 &= 5.32 & \tau_2 &= 626.4 \\ p_3 &= 4 \end{aligned}$$

$$\begin{aligned} U_1 D_1 &= 772 \times .2627 \left\{ 799 - 626.4 \left( 1 + \text{hyp log } \frac{799}{626.4} \right) \right\} \\ &+ \frac{799 - 626.4}{799} \cdot 177760 + 20 (5.32 - 4) 144 = 46478 \end{aligned}$$

$$H_1 D_1 = 772 \times .2627 (799 - 561.2) + 177760 = 225986$$

$$\text{Eff.} = \frac{U_1 D_1}{H_1 D_1} = \frac{46478}{225986} = .2073$$

*Case 2.*

$$\begin{aligned} p_1 &= 115 & \tau_1 &= 799 & r &= 10 \\ p_2 &= 8.907 & \tau_2 &= 648.5 \\ p_3 &= 4 \end{aligned}$$

$$\begin{aligned} U_1 D_1 &= 722 \times .2627 \left[ 799 - 648.5 \left( 1 + \text{hyp log } \frac{799}{648.5} \right) \right] \\ &+ \frac{799 - 648.5}{799} \cdot 177760 + 10 (8.907 - 4) 144 = 43715 \end{aligned}$$

$$\text{Eff.} = \frac{43715}{225986} = .1934$$

*Case 3.*

$$\begin{aligned} p_1 &= 115 & \tau_1 &= 799 & r &= 6.66 \\ p_2 &= 13.98 & \tau_2 &= 670.7 \\ p_3 &= 4 \end{aligned}$$

$$\begin{aligned} U_1 D_1 &= 772 \times .2627 \left[ 799 - 670.7 \left( 1 + \text{hyp log } \frac{799}{626.4} \right) \right] \\ &+ \frac{799 - 670.7}{799} \cdot 177760 + 6.66 (13.98 - 4) 144 = 40481 \end{aligned}$$

$$Eff. = \frac{40481}{225986} = .1795$$

Case 4.

$$\begin{array}{lll} p_1 = 115 & \tau_1 = 799 & r = 4 \\ p_2 = 24.64 & \tau_2 = 700.2 & \\ p_3 = 4 & & \end{array}$$

$$U_1 D_1 = 772 \times .2627 \left[ 799 - 700.2 \left( 1 + \text{hyp log } \frac{799}{700.2} \right) \right]$$

$$+ \frac{799 - 700.2}{799} \cdot 177760 + 4 (24.64 - 4) 144 = 35305$$

$$Eff. = \frac{35305}{225986} = .1566$$

Case 5.

$$\begin{array}{lll} p_1 = 115 & \tau_1 = 799 & r = 2.857 \\ p_2 = 35.85 & \tau_2 = 721.7 & \\ p_3 = 4 & & \end{array}$$

$$U_1 D_1 = 772 \times .2627 \left\{ 799 - 721.7 \left( 1 + \text{hyp log } \frac{799}{721.7} \right) \right\}$$

$$+ \frac{799 - 721.7}{799} \cdot 177660 + 2.857 (35.82 - 4) 144 = 30700$$

$$Eff. = \frac{30700}{225986} = .1358$$

Case 6.

$$\begin{array}{lll} p_1 = 115 & \tau_1 = 799 & r = 2.22 \\ p_2 = 47.41 & \tau_2 = 738.8 & \\ p_3 = 4 & & \end{array}$$

$$U_1 D_1 = 772 \times .2627 \left\{ 799 - 738.8 \left( 1 + \text{hyp log } \frac{799}{738.8} \right) \right\}$$

$$+ \frac{799 - 738.8}{799} \cdot 177760 + 2.22 (47.41 - 4) 144 = 27959$$

$$Eff. = \frac{27959}{225986}$$

Case 7.

$$\begin{array}{lll} p_1 = 115 & \tau_1 = 799 & r = 1.818 \\ p_2 = 39.19 & \tau_2 = 752.6 & \\ p_3 = 4 & & \end{array}$$



$$U_1 D_1 = 77.2 \times .2627 \left[ 799 - 752.6 \left( 1 + \text{hyp log } \frac{799}{752.6} \right) \right] \\ + \frac{799 - 752.6}{799} \times 177760 + 1.818 (59.19 - 4) 144 = 25298$$

$$\text{Eff.} = \frac{25298}{225986} = .1119$$

Case 8.

$$\begin{array}{lll} p_1 = 115 & \tau_1 = 799 & r = 1.33 \\ p_2 = 83.77 & \tau_2 = 776.2 & \\ p_3 = 4 & & \end{array}$$

$$U_1 D_1 = 77.2 \times .2627 \left[ 799 - 776.2 \left( 1 + \text{hyp log } \frac{799}{776.2} \right) \right] \\ + \frac{799 - 776.2}{799} \times 177760 + 1.33 (83.77 - 4) 144 = 20344$$

$$\text{Eff.} = \frac{20344}{225986} = .0898$$

Case 9.

$$\begin{array}{lll} p_1 = 115 & \tau_1 = 799 & r = 1 \\ p_2 = 115 & \tau_2 = 799 & \\ p_3 = 4 & & \end{array}$$

$$U_1 D_1 = 77.2 \times .2627 \left[ 799 - 799 \left( 1 + \text{hyp log } \frac{799}{799} \right) \right] \\ + \frac{799 - 799}{799} \times 1 (115 - 4) 144 = 15984$$

$$\text{Eff.} = \frac{15984}{225986} = .0707$$

# COMPUTATIONS—IDEAL CASES.

## *With Jacket.*

Case 1.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & V_1 = 3.806 & r = 20 \\ p_2 = 5.63 & \tau_2 = 628.9 & V_2 = 76.12 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \text{ hyp log } \frac{799}{628.9} - b (799 - 628.9) + 76.12 (5.63 - 4) 144 \\ = 191170$$

$$h = 772 (628.9 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{628.9} \right) - b \cdot 799$$

$$= 99529$$

$$\text{Eff. } \frac{u}{h} = \frac{191170}{995290} = .193$$

Case 2.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & I_1 = 3.806 & r = 10 \\ p_2 = 9 & \tau_2 = 649.5 & I_2 = 38.06 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \text{ hyp log } \frac{799}{649.5} - b (799 - 649.5) + 38.06 (9 - 4) \cdot 144$$

$$h = 772 (649.5 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{649.5} \right) - b \cdot 799$$

$$= 975517$$

$$\text{Eff.} = \frac{1763.02}{9755.17} = .1808$$

Case 3.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & v_1 = 3.806 & r = 6.66 \\ p_2 = 15.33 & \tau_2 = 649.5 & v_2 = 25.34 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \text{ hyp log } \frac{799}{675.4} - b (799 - 675.4) + 25.34 (15.33 - 4) \cdot 144$$

$$= 158179$$

$$h = 772 (675.4 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{675.4} \right) - b \cdot 799$$

$$= 949644$$

$$\text{Eff.} = \frac{158179}{949644} = .1665$$

Case 4.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & I_1 = 3.806 & r = 4 \\ p_2 = 26.36 & \tau_2 = 704.2 & I_2 = 15.22 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \text{ hyp log } \frac{799}{704.2} - b (799 - 704.2) + 15.22 (26.36 - 4) \cdot 144$$

$$= 133244$$

$$h = 772 (704.2 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{704.2} \right) - b. 799$$

$$= 923856$$

$$\text{Eff.} = \frac{133244}{923856} = .1442$$

Case 5.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & V_1 = 3.806 & r = 2.857 \\ p_2 = 37.69 & \tau_2 = 724.7 & V_2 = 10.87 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \log \frac{799}{724.7} - 540.4 (799 - 724.7)$$

$$+ 10.87 (37.69 - 4) 144 = 118321$$

$$h = 772 (724.7 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{724.7} \right) - b. (799)$$

$$= 909723$$

$$\text{Eff.} = \frac{118321}{909723} = .2301$$

Case 6.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & V_1 = 3.08 & r = 2.22 \\ p_2 = 49.28 & \tau_2 = 740.8 & V_2 = 8.449 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \text{hyp log } \frac{799}{740.8} - b (799 - 740.8) + 8.449 (49.28 - 4) 144$$

$$= 108963$$

$$h = 772 (740.8 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{740.8} \right) - b (799)$$

$$= 901746$$

$$\text{Eff.} = \frac{108963}{901746} = .1208$$

Case 7.

$$\begin{array}{llll} p_1 = 115 & \tau_1 = 799 & V_1 = 3.806 & r = 1.818 \\ p_2 = 60.93 & \tau_2 = 753.7 & V_2 = 6.919 & \\ p_3 = 4 & & & \end{array}$$

$$U = a \text{hyp log } \frac{799}{753.7} - b (799 - 753.7) + 6.919 (60.93 - 4) 144$$

$$= 96883$$

$$h = 772 \times (753 - 561) + a \left( 1 + \text{hyp log } \frac{799}{753} \right) - b \cdot 799$$

$$= 890956$$

$$\text{Eff.} = \frac{96883}{890956} = .1087$$

Case 8.

$$p_1 = 115 \quad \tau_1 = 799 \quad V_1 = 3.806 \quad r = 1.33$$

$$p_2 = 85 \quad \tau_2 = 777.2 \quad V_2 = 5.161$$

$$p_3 = 4$$

$$U = a \text{ hyp log } \frac{799}{777.2} - b (799 - 777.2) + 5.161 (8.5 - 4) \cdot 144$$

$$= 70387$$

$$h = 772 (777.2 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{777.2} \right) - b \cdot 799$$

$$= 86649$$

$$\text{Eff.} = \frac{70387}{866490} = .0812$$

Case 9.

$$p_1 = 115 \quad \tau_1 = 799 \quad V_1 = 3.806 \quad r = 1$$

$$p_2 = 115 \quad \tau_2 = 799 \quad V_2 = 3.806$$

$$p_3 = 4$$

$$U = a \text{ hyp log } \frac{799}{799} - b (799 - 799 + 3 \cdot 806 (115 - 4) \cdot 144$$

$$h = 772 (799 - 561.2) + a \left( 1 + \text{hyp log } \frac{799}{799} \right) - b \cdot 799$$

$$= 861350$$

$$\text{Eff.} = \frac{60835}{861350} = .0707$$

Tabulating the results as obtained under the several cases, we have

#### EFFICIENCIES OF WORKING FLUID.

##### Steam engine, Jacketed and Unjacketed.

Cut-off.	Ratio Exp.	Eff. Without.	Eff. with Jacket.
0.05	20.00	0.2073	0.1930
.10	10.00	.1934	.1808
.15	6.66	.1795	.1665
.25	4.00	.1566	.1442
.35	2.85	.1358	.1302
.45	2.22	.1237	.1209
.55	1.82	.1119	.1087
.75	1.33	.0898	.0812
1.00	1.00	.0707	.0707

An examination of the tables, of the curves still better, will show clearly the wasteful influence of the steam jacket, as an element considered by itself. Within the usual range of practice, from about five or six to fifteen or twenty expansions, under the assumed conditions of initial pressure and cut-off, it is seen that the loss by its application is fairly constant at something above one per cent., in these cases; rapidly falling to zero as the ratio of expansion falls from the lower figures to unity. The consumption of steam in pounds per horse-power per hour, may be computed very approximately by dividing 2·5 by the computed efficiencies. The cases assumed are for condensing engines, and the evaporation always taken at nine pounds of steam per pound of fuel, the fuel expenditure may be gauged by dividing the weight of steam computed by nine. This gives, for example, about 12·06 and 12·95 pounds for the unjacketed and for the jacketed engine, respectively, at a ratio of twenty, in steam demanded: and of about 1·33 and 1·44 pounds of fuel. For a ratio of expansion of four, the figures become about 16 and 17·3 respectively, for the steam and 1·75 and 1·85 pounds of fuel. At full stroke, the figures become 35 pounds of steam and of feed water, and 4 pounds of fuel per horse-power and per hour, for both engines.

We conclude, from the above, therefore, that, in the ideal case, the steam jacket reduces efficiency, necessarily and without exception, and that, for ordinary variations of the ratio of expansion for the pressure here assumed, this waste is a nearly constant fraction. To put it more mathematically, the gain is negative, and nearly constant at about one per cent. efficiency.

(2) REAL EFFICIENCIES.—In 1886, a "Research Committee" was appointed by the British Institution of Mechanical Engineers, to investigate the subject of the steam jacket.\* A very unusually complete set of data, pertaining to trials made with a view to determine the efficiency produced by application of the jacket was secured, and from these the following figures were collated and results deduced:

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\* Proceedings, 1890.



(1) The case of a single-cylinder non-condensing Corliss engine, 21'65 x 43'31 inches, the body only jacketed. The jackets were supplied by a small pipe from the main steam pipe and were automatically drained.

Following are the data as obtained, the experiments being carried on at variable boiler pressures and ratios of expansion. The comparisons to be given, like the preceding, were made, and reported to the writer, by Messrs. Hitchcock and Mount.

#### STEAM-JACKET EFFICIENCIES.

(See Curves 1, Set I, Fig. II.)

	<i>Without.</i>	<i>With.</i>
Jackets, . . . . .	—	—
Boiler pressure above atmos., . . . . .	110	110
Expansions, . . . . .	6'2	7'1
Piston speed, . . . . .	445	439
Feed water per I. H. P. per hr., . . . . .	28'38	22'80
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	6'2	6'2
Piston speed, . . . . .	445	447'5
Feed water, . . . . .	28'38	22'13
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	5	5'3
Piston speed, . . . . .	443	447'5
Feed water, . . . . .	26'82	22'35
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	4'4	4'4
Piston speed, . . . . .	459	453
Feed water, . . . . .	27'58	23'65

(Curves 2, Set I, Fig. II.)

Jackets, . . . . .	78'2	7
Boiler pressure, . . . . .	78'2	78'2
Expansions, . . . . .	5'4	5'1
Piston speed, . . . . .	447	441
Feed water, . . . . .	27'58	23'65
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	78'2	78'2
Expansions, . . . . .	4'8	5'1

STEAM-JACKET EFFICIENCIES.—(*Continued.*)

	<i>Without.</i>	<i>With.</i>
Piston speed, . . . . .	440	441
Feed water, . . . . .	26'71	23'65
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	78'2	78'2
Expansions, . . . . .	3'7	3'8
Piston speed, . . . . .	433	445
Feed water, . . . . .	24'58	22'58
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	78'2	78'2
Expansions, . . . . .	2'9	3'1
Piston speed, . . . . .	437	437
Feed water, . . . . .	24'18	21'95

(See Curves 3, Set I, Fig. II.)

Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49'8	49'8
Expansions, . . . . .	3'7	3'9
Piston speed, . . . . .	443	437
Feed water, . . . . .	27'27	25'3
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49'8	49'8
Expansions, . . . . .	2'5	2'8
Piston speed, . . . . .	441	437
Feed water, . . . . .	27'2	25'21
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49'8	49'8
Expansions, . . . . .	1'7	1'7
Piston speed, . . . . .	440	440
Feed water, . . . . .	30'17	28'74
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49'8	49'8
Expansions, . . . . .	1'	1'
Piston speed, . . . . .	437	441
Feed water, . . . . .	46'82	46'26

Curves *A B C* on *Fig. IV*, are derived from curves 1, 2, 3, *Set I* on *Fig. II*, by determining the efficiency of the jacket for different ratios of expansion in the following manner: A certain ratio of expansion was taken and, by comparing the amounts of feed water, as read from the curves, for that

ratio, we determine the difference in efficiency due to the jacket for that expansion.

These curves show an increased efficiency with increase in ratio of expansion.

The second case is that of a single cylinder condensing engine (Corliss), cylinder dimensions same as before, body only jacketed ; experiments carried on at the same place, in the same year and in the same manner.

The following are the data as obtained :

#### STEAM-JACKET EFFICIENCIES.

(See Curves 1, Set II, Fig. II.)

	<i>Without.</i>	<i>With.</i>
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	13'5	11'3
Piston speed, . . . . .	433	424
Feed water, . . . . .	23'24	17'05
Feed water ideal case, . . . . .	15'63	17'00

Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	10'7	10
Piston speed, . . . . .	423	444
Feed water, . . . . .	22'24	16'65
Feed water ideal, . . . . .	17'00	17'30

Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	8'2	10
Piston speed, . . . . .	429	432
Feed water, . . . . .	21'41	16'30
Feed water ideal, . . . . .	17'70	17'30

Jackets, . . . . .	—	—
Boiler pressure, . . . . .	110	110
Expansions, . . . . .	6'4	6'4
Piston speed, . . . . .	416	419
Feed water, . . . . .	21'99	17'59
Feed water ideal, . . . . .	17'60	18'94

(See Curves 2, Set II, Fig. II.)

Jackets, . . . . .	—	—
Boiler pressure, . . . . .	88'9	88'9
Expansions, . . . . .	9'3	12
Piston speed, . . . . .	432	430
Feed water, . . . . .	21'19	17'66

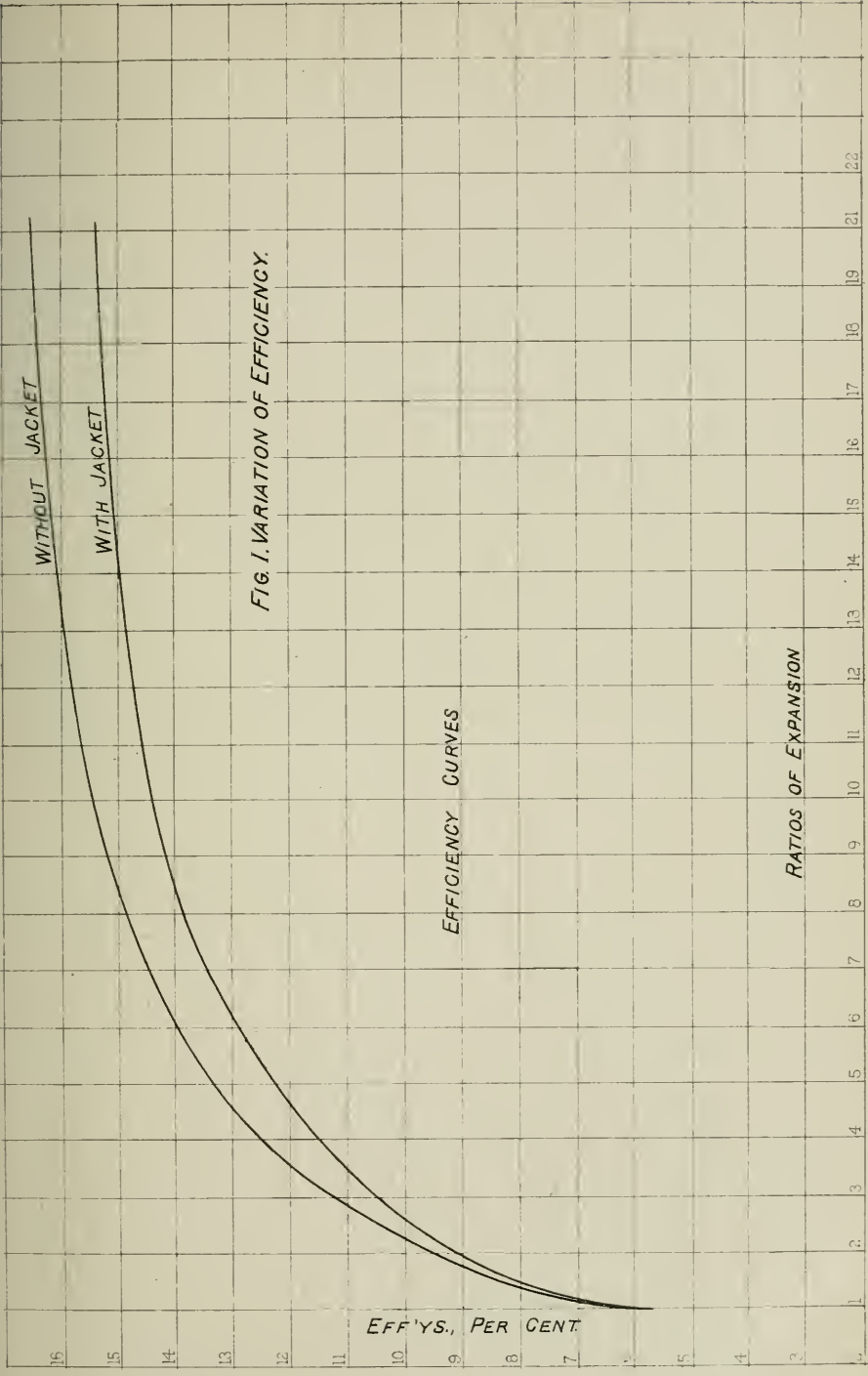


FIG. 1. VARIATION OF EFFICIENCY.









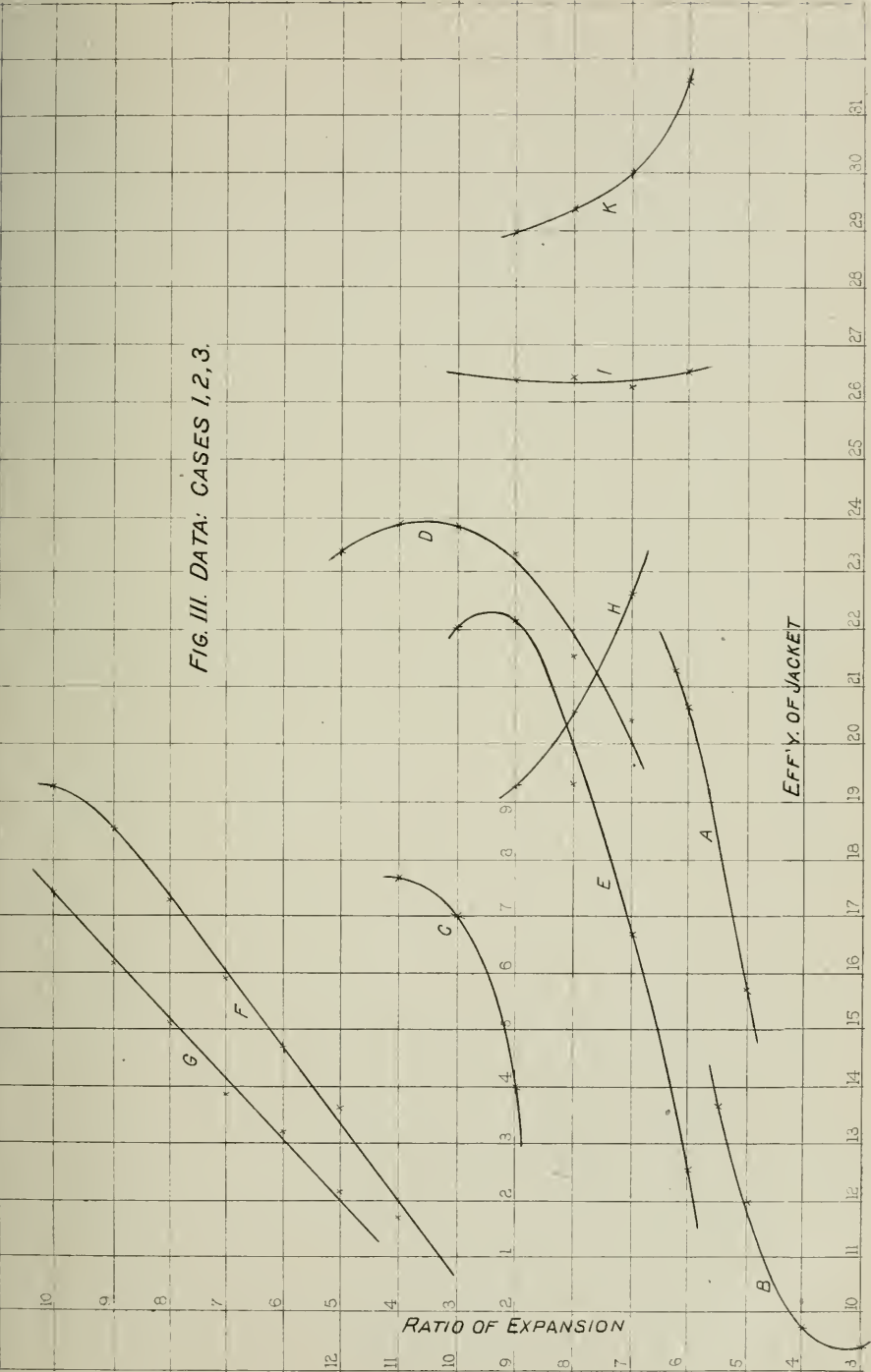
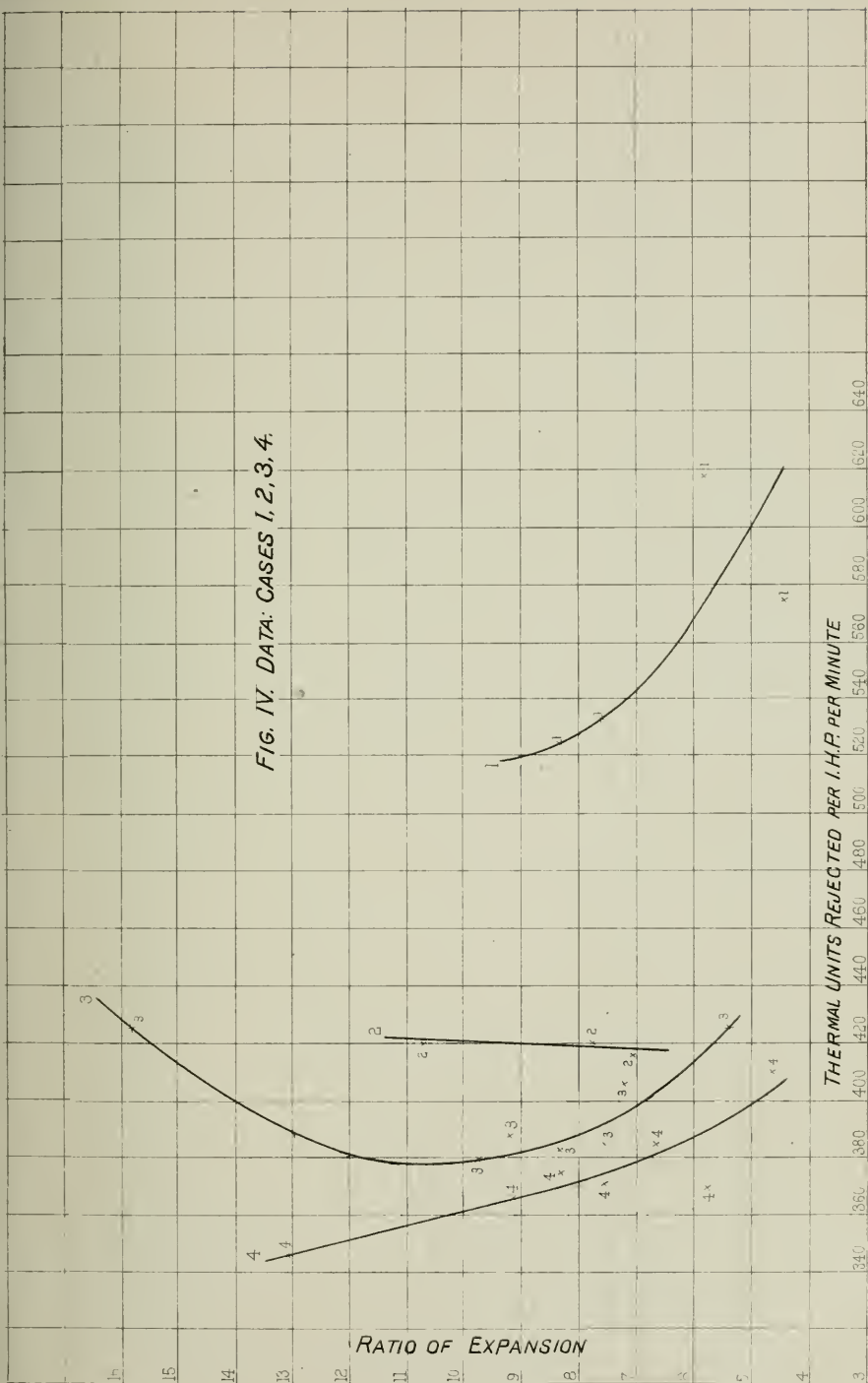
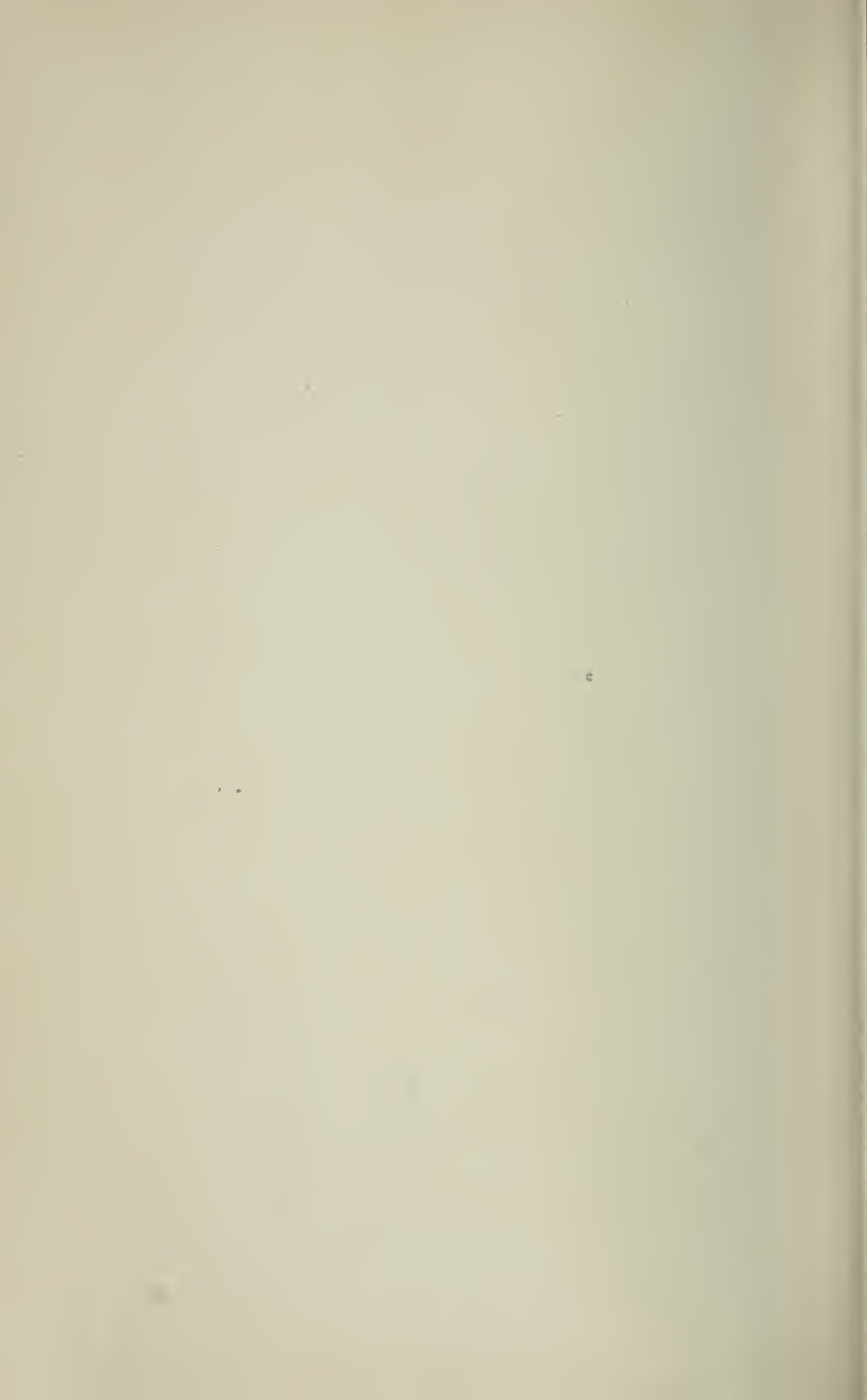




FIG. IV. DATA: CASES 1, 2, 3, 4.







## STEAM-JACKET EFFICIENCIES.—(Continued.)

	<i>Without.</i>	<i>With.</i>
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	88·9	88·9
Expansions, . . . . .	8·7	11·3
Piston speed, . . . . .	428	430
Feed water, . . . . .	21·05	17·32
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	88·9	88·9
Expansions, . . . . .	7·3	6·8
Piston speed, . . . . .	432	429
Feed water, . . . . .	20·79	16·87
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	88·9	88·9
Expansions, . . . . .	5·9	5·9
Piston speed, . . . . .	419	419
Feed water, . . . . .	19·89	17·5
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	88·9	88·9
Expansions, . . . . .	4·8	5·9
Piston speed, . . . . .	427	433
Feed water, . . . . .	20·43	17·50
(See Curves 3, Fig. III; Set II, Fig. II.)		
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	64	64
Expansions, . . . . .	10·8	10·7
Piston speed, . . . . .	4·21	4·32
Feed water, . . . . .	20·41	18·48
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	64	64
Expansions, . . . . .	8·2	8·2
Piston speed, . . . . .	429	430
Feed water, . . . . .	19·09	17·59
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	64	64
Expansions, . . . . .	5·4	5·4
Piston speed, . . . . .	426	424
Feed water, . . . . .	18·06	17·34
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	64	64
Expansions, . . . . .	3·9	4·4
Piston speed, . . . . .	421	427
Feed water, . . . . .	18·37	17·66

## STEAM-JACKET EFFICIENCIES.—(Continued.)

	<i>Without.</i>	<i>With.</i>
Jackets, . . . . .	64	64
Boiler pressure, . . . . .	64	64
Expansions, . . . . .	3.6	3.6
Piston speed, . . . . .	427	426
Feed water, . . . . .	18.84	18.55
(See Curves 4, Set II, Fig. II.)		
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49.8	49.8
Expansions, . . . . .	10.7	12
Piston speed, . . . . .	438	435
Feed water, . . . . .	20.65	19.29
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49.8	49.8
Expansions, . . . . .	5.6	5.6
Piston speed, . . . . .	436	431
Feed water, . . . . .	18.77	18.17
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49.8	49.8
Expansions, . . . . .	7.9	7.6
Piston speed, . . . . .	424	416
Feed water, . . . . .	19.36	18.48
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49.8	49.8
Expansions, . . . . .	4.2	4
Piston speed, . . . . .	424	434
Feed water, . . . . .	19	18.91
Jackets, . . . . .	—	—
Boiler pressure, . . . . .	49.8	49.8
Expansions, . . . . .	3.2	3.2
Piston speed, . . . . .	424	432
Feed water, . . . . .	19.78	19.67

Curves constructed as before are here marked *D E F* and *G* on *Fig. IV*; being derived from 1, 2, 3 and 4, Set II, *Fig. II*. These curves also show that the efficiency of the jacket with such engines increases with increase in ratio of expansion, and in the case of *D* and *E* increases up to a certain value of the ratio of expansion and then decreases.

The third case applies to a horizontal compound con-

densing tandem engine, the body of the cylinders only being jacketed. The whole steam supply to the cylinders passed through the jackets, which were drained by trap; and when not in use the jackets were open to the air.

The trials were carried on at a constant boiler pressure of forty-two pounds above atmosphere and a piston speed of 196 feet per minute.

#### STEAM-JACKET EFFICIENCIES.

##### *Without Steam in either Jacket.*

(See Curves 1, Fig. III.)

Expansions, . . . . .	8.38	7.54
Thermal units rejected per I. H. P. per m., . .	5.25	5.33
Expansions, . . . . .	5.8	4.45
Thermal units rejected per I. H. P. per m., . .	6.19	5.74

##### *With Steam in High-pressure Jacket Alone.*

(See Curves 2, Fig. III.)

Expansions, . . . . .	10.78	7.73	7.01
Thermal units rejected, . . . . .	4.20	4.20	4.18

##### *With Steam in Low-pressure Jacket Alone.*

(See Curves 3, Fig. III.)

Expansions, . . . . .	15.8	9.74	9.14
Thermal units rejected, . . . . .	4.27	3.80	3.88
Expansions, . . . . .	8.38	7.54	
Thermal units rejected, . . . . .	3.83	3.85	
Expansions, . . . . .	7.18	5.38	
Thermal units rejected, . . . . .	4.07	4.27	

##### *With Steam in both Jackets.*

(See Curves 4, Fig. III.)

Expansions, . . . . .	13.1	13.1	8.38
Thermal units rejected per I.H.P. per m.,	3.47	3.48	3.75
Expansions, . . . . .	9.14	7.54	6.7
Thermal units rejected, . . . . .	3.67	3.72	3.85
Expansions, . . . . .	5.8	4.64	
Thermal units rejected, . . . . .	3.70	4.10	

## COMPUTED DATA, BY WHICH CURVES ON FIG. IV ARE PLOTTED.

<i>Ratio of exp. Curve.</i>	<i>Eff. of jacket. Per cent.</i>	<i>Ratio of exp. Curve.</i>	<i>Eff. of jacket. Per cent.</i>
A 6.2	21.33	F 8	7.38
6	20.61	7	5.94
5	15.79	6	4.67
B 5.5	13.6	5	3.6
5	12	4	1.64
4	9.63	G 10	7.45
3	9.38	9	6.28
C 4	7.64	8	5.13
3	7	7	3.84
2	3.93	6	3.08
D 12	23.43	5	2.03
11	23.82	H 9	19.23
10	23.78	8	20.45
9	23.2	7	22.55
8	21.5	I 9	26.34
7	20.32	8	26.34
E 10	21.95	7	26.24
9	22.14	6	26.5
8	19.23	K 9	29
7	16.66	8	29.35
6	12.34	7	30
F 10	9.25	6	31.62
9	8.62		

The preceding work, including the computations and all the comparisons of data, has been performed with great care, the two computers checking the figures and the results, in such manner that, as shown by the forms of the curves, there can be no doubt of the existence of this maximum in the value of the steam jacket, the ratios of expansion being varied as seen, and it is probably fairly to be assumed that it may be found in all cases. These data and deductions will well repay a much more extended study than it is here practicable to give them. A short review of the work will, however, reveal some interesting and probably important facts.

In the first case, that of the simple non-condensing Corliss engine, the heads unjacketed, we see, taking the first example, that the use of the jacket reduced the cylinder-wastes from about twenty-five per cent. of the ideal consumption of steam and feed water to about half that pro-



portion, for ratios of expansion approximating six; from one-third to about one-tenth, at a ratio of five; and apparently from twenty to ten per cent. at 4.4. The same general effect is observed throughout, with some discrepancies which may be due either to varying action of the jacket or to slight errors of observation, or to both combined, the latter being the probable fact.

In this first case, also, it will be observed that the jacket gives best results, with 110 pounds of steam, when the ratio of expansion approximates six. When the steam pressure falls to approximately eighty pounds, the best work of the jacket occurs at a ratio not far from 4.75; while, at the pressure of fifty pounds, the value of the jacket increases through the whole range of the experiments, and not only so, but the curve assumes a rectilinear form, indicative of probable improvement indefinitely in the direction of increasing expansion. The highest efficiencies, however, either with or without the jacket are found, in this case, at the lowest ratios adopted, and indicate a maximum value at about 3.25. The ratios of expansion for maximum efficiency of fluid, in the other cases, are seen to be, for 110 pounds, about five, and for eighty pounds, about 3.5.

Similarly studying the performance of the condensing engine, we find that the best work done, whether jacketed or not, at about a ratio of expansion of ten (at a steam-pressure of 110 pounds), but that the jacketed engine reduces the internal wastes from fifty per cent. at highest ratios, and from one-fourth at the lowest ratios, in the case of the unjacketed engine, to five per cent., and, in some cases, probably to within the magnitude of the errors of observation. At a pressure of ninety pounds the best ratio seems to be for this engine, under the given conditions of operation, about 6.5 when unjacketed, and 8.5 jacketed; while the lower pressures still further reduce both the efficiencies and the savings effected by the jacket. The best work of the jacket, as an economizer of heat, is done at high pressure, at a ratio of expansion of twelve or more. In all cases it seems to be the fact, with these engines at least, that the jacket is useful beyond the ratios of maximum efficiency of fluid.

The compound engine is operated at altogether too low a pressure to bring out the best effect of compounding; but it exhibits the same general effects which have been noted in the cases of working of simple engines. The effect of the jacket is less pronounced than in the simple engine, and the efficiencies of fluid vary less with variation of the ratios of expansion. It gives its best result at ratios of expansion ranging from 7.5 to 10.5, the variations of the value being very much more observable in the last case, in which both jackets are in use, than in either of the others, and least in Case 3, in which only the high-pressure jacket was employed. By far the best work was done by the engine when both jackets were in use.

This discovery of a maximum efficiency of jacket may throw some light upon the causes of the conflicting and sometimes apparently irreconcilable results of trials of engines with and without jackets, and with jackets variously constructed. The discovery may also prove of value to the designer, as aiding him in securing the best proportions and arrangement of his engine.

PROCEEDINGS  
OF THE  
CHEMICAL SECTION  
OF THE  
FRANKLIN INSTITUTE.

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*[Stated meeting, held at the institute, Tuesday, March 17, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, March 17, 1891.

Dr. WM. H. WAHL, president, in the chair.

There were present fifteen members and four visitors.

Letters from Dr. F. A. Geuth and Prof. Joseph W. Richards, acknowledging election to membership, were read.

Mr. George H. Bartram read an interesting paper on "A source of error in the phenol-sulphuric acid method of determining nitrates in potable waters."

The paper was commented upon by Mr. Reuben Haines, who remarked that in his experience in the use of this method he had suspected the existence of such a source of error as was pointed out in Mr. Bartram's paper.

Mr. Fred. E. Ives read a paper criticising adversely the results obtained by Prof. Lippmann in photographing colors. The paper was discussed by Messrs. Hall, Wahl, Jayne and Haines; considerable interest was manifested in the paper.

Mr. Reuben Haines then read a paper, entitled "Percentage of iodine absorbed by lard oil."

Mr. H. Pemberton, Jr., presented two papers, entitled, respectively, "Analysis of a chromite" and "An apparatus for heating substances in glass tubes;" they were read by title. The papers presented were referred for publication.

Adjourned.

WM. C. DAY, *Secretary.*

## THE ELECTROLYTIC METHOD APPLIED TO RHODIUM.

BY EDGAR F. SMITH.

[Read at the stated meeting, held February 17, 1891.]

So far as I have been able to learn, the salts of rhodium have never been subjected to the action of the electric current. I, therefore, made experiments in this direction and obtained results indicating the possibility of employing the electrolytic method in the estimation of this metal. The various texts consulted led me to believe that in all likelihood a solution of the double cyanide of rhodium and an alkali metal would best answer my purposes. Precipitation did not occur upon mixing solutions of potassium cyanide and rhodio-sodium chloride. The liquid remained clear. As soon, however, as a comparatively feeble current acted upon the double cyanide, a reddish yellow colored compound appeared, and remained suspended in the liquid. It was probably a cyanide which sustained no change. I did not attempt a repetition of this experiment.

From my experience in the electrolysis of gold, platinum and palladium solutions containing alkaline phosphates and free phosphoric acid, I concluded to try rhodium under similar conditions. Sodium rhodium chloride,  $\text{Na}_6\text{Rh}_2\text{Cl}_6 + 24\text{H}_2\text{O}$ , was the salt used by me. A sufficient quantity of it was dissolved so that 10 cc. of liquid contained, by calculation, 0.0980 gram of metallic rhodium.

*Experiments.*—(1) To 10 cc. of the rhodium solution were added 30 cc. of sodium phosphate,  $\text{Na}_2\text{HPO}_4$  (sp. gr. 1.0358), and 3 cc. of phosphoric acid (sp. gr. 1.347). The total dilution of the electrolyte was 180 cc. The current gave 1.8 cc. OH gas per minute. In seven hours the deposition of metal was finished.

$$\begin{array}{rcl}
 \text{Weight of crucible + rhodium} & = & 63.4765 \\
 \text{" " " alone,} & = & 63.3783 \\
 \hline
 \text{Rhodium} & = & 0.0982
 \end{array}$$

The deposition of metal occurred at the ordinary temperature. At the beginning of the decomposition the liquid showed a beautiful deep purple color, but as the metal separated, it rapidly became lighter and finally colorless. Upon inclining the dish in which the precipitation was made, thus exposing a fresh metal surface, the latter remained perfectly clear. The precipitation of metal was completed.

(2) The conditions here were nearly the same as in the preceding trial. The total dilution of liquid equalled 200 cc. while the current gave 1.6 cc. of OH gas per minute.

The metallic rhodium was precipitated upon copper-plated platinum dishes. It was rather black in color, very compact and perfectly adherent. It was washed without any difficulty. Hot water was used for this purpose. The drying was done upon a warm iron plate.

The results obtained accord so well with the theory that the accuracy of the method cannot be questioned. The rapidity with which the metal is deposited, and the ease with which it may be handled also recommend this method of estimation.

CHEMICAL LABORATORY OF THE UNIV. OF PA.,  
PHILADELPHIA, February 13, 1891.

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## THE ELECTROLYTIC DETERMINATION OF MERCURY.

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BY EDGAR F. SMITH.

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[*Read at the stated meeting, held February 17, 1891.*]

In gravimetric analysis mercury is frequently obtained as sulphide. To weigh it as such, or to convert it into some form suitable for weighing, requires much time and close attention to insure results that will be at all satisfactory. It is, therefore, better to have recourse to the electrolytic method of determination. As mercury sulphide dissolves quite readily in the fixed alkaline sulphides, I electrolyzed such solutions. The results show that this



procedure affords a very accurate and clean method for the estimation of this metal.

The sodium sulphide was prepared as described in the paper relating to the determination of gold. Its specific gravity was 1.19.

Solutions of mercuric chloride, containing a known amount of metal, were first tried. The table shows the conditions of experiment and the results:

Mercury present in grams	Sodium sulphide present. Sp. gr. 1.19.	Total dilution.	Current in cc. OH gas per minute.	Mercury found.
0.1903	20 cc.	125 cc.	1 cc.	0.1902
0.1903	20 cc.	125 cc.	1 cc.	0.1905
0.1903	20 cc.	125 cc.	1 cc.	0.1907
0.1903	20 cc.	130 cc.	1.2 cc.	0.1910
0.1903	30 cc.	130 cc.	1.2 cc.	0.1900

The precipitation extended through the night. The deposited mercury was gray in color and very compact. Only once was a tendency to the fluid form observed. Hot water was used for washing. The metal was dried by holding the platinum dish in the palm of the hand and blowing gently upon the deposit.

If mercury and arsenic are both present in a solution of sodium sulphide, the current will throw out the former. It will not carry down any arsenic. This is evident from the two analyses that follow:

Mercury present.	Arsenic present.	Sodium sulphide.	Total dilution.	Current in cc. OH gas per minute.	Mercury found.
0.1903 gr.	100 %	25 cc.	125 cc.	2 cc.	0.1908 gr.
0.1903 gr.	100 %	25 cc.	125 cc.	2 cc.	0.1894 gr.

In the communication upon the deposition of gold from sodium sulphide solutions, mention is made that gold and tin could not be separated electrolytically when present together in a solution of that kind. I was, therefore, rather surprised to discover that mercury could be separated from

tin under such circumstances. The conditions of experiment were similar to the following:

The solution contained 0.1903 gram of mercury, 0.1200 gram of tin, and 30 cc. of sodium sulphide. The total dilution was 125 cc. The current gave 2 cc. of OH gas per minute, and acted for twelve hours. The precipitated mercury weighed 0.1909 gram.

The filtrates from the mercury deposits did not show even traces of this metal when they were examined.

CHEMICAL LABORATORY OF THE UNIV. OF PA.,  
PHILADELPHIA, February 13, 1891.

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## THE ELECTROLYTIC DETERMINATION OF GOLD.

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BY EDGAR F. SMITH.

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[*Read at the stated meeting, held February 17, 1891.*]

Gold has been estimated electrolytically in solutions, containing it as a double cyanide, or in the presence of sodium phosphate and phosphoric acid. From recent experiments I am satisfied it can be determined in the same manner by the decomposition of solutions of sodium sulphaurate.

Years ago Parodi and Mascazzini (*Zeit. f. Analyt. Chemie*, **18**, 588), showed that antimony was completely precipitated from solutions of its ammonium sulpho-salt by the electric current. Classen elaborated this idea (*Ber.* **14**, 1622; **17**, 2467; **17**, 2245; **18**, 1110), substituting sodium sulphide for ammonium sulphide, and succeeded in effecting a most excellent separation of antimony from both tin and arsenic.

The sodium sulphide, used in the experiments recorded in this communication, was prepared by supersaturating a definite volume of caustic soda (1.3 sp. gr.) with hydrogen sulphide gas. An equal amount of caustic soda was then added, and the current of hydrogen sulphide continued for some hours through the solution. The liquid was then rapidly concentrated, until a crystalline scum appeared upon its surface. In this condition it was placed in bottles

provided with tight-fitting corks. Crystals separated as the liquid cooled. These were recrystallized, and then dissolved for use. The specific gravity of their solution equalled 1.1800.

*Experiments.*—(1) To a chloride solution of gold containing 0.1446 gram of metallic gold, were added 10 cc. of sodium sulphide ( $\text{Na}_2\text{S}$ ), and 100 cc. of water. The electric current, allowed to act upon this mixture, gave 2.6 cc. OH gas per minute. The time of precipitation extended through the night.

The deposit of metallic gold weighed 0.1446 gram. It was bright yellow in color, and very adherent. Water alone was used to wash it. The drying was done upon a warm iron plate.

(2) In this experiment the same quantity of gold was present as in the first trial. The volume of sodium sulphide did not exceed 20 cc., while the total dilution of the electrolyte was 100 cc. The current gave 2.6 cc. OH gas per minute. The metallic deposit was bright in appearance, firm and adherent. It weighed 0.1446 gram. The precipitation was complete in five and one-half hours.

The depositions of metal were made directly upon platinum. The gold was removed by covering it with a dilute solution of potassium cyanide, allowing the latter to remain for several hours in the dishes. When desired, the gold deposit can be very speedily removed by connecting the dish with the anode of a battery, and employing a platinum rod, which dips into the solution, as cathode. Platinum will not dissolve if feeble currents are used.

The results, given above, leave no doubt as to the applicability of this method in the estimation of gold.

Subsequent efforts were directed to its separation from other metals. As antimony can be deposited from a sodium sulphide solution by a current giving 1.5–2.0 cc. of OH gas per minute, I made no attempt to separate it from gold. The separation of gold from tin was much desired. Numerous trials were made. The results, however, were unsatisfactory. The gold was, in all instances, precipitated, but it carried down with it from one to three per cent. of tin.

The sodium sulphide, in these separations, showed a specific gravity of 1.21. An additional gram of sodium hydroxide was introduced into the electrolyte for every centigram of metallic tin present. The current did not exceed 1.5 cc. of OH gas per minute. The dilution with water was 100 cc. While these conditions were exactly analogous to those in which antimony is separated from tin, they failed in the case of gold and tin.

*Gold from Arsenic.*—(1) The solution electrolyzed contained 0.1446 gram of gold, as chloride, 20 cc. of sodium arsenate solution (= 0.15920 gr. metallic arsenic), 60 cc. sodium sulphide, and 100 cc. of water. The current gave 1.2 cc. OH gas per minute. It ran through the night. The washed and dried deposit of gold weighed 0.1443 gram.

(2) With conditions similar to those in (1) the gold weighed 0.1446 gram.

*Gold from Molybdenum.*—(1) 0.1437 gram of gold, as chloride, 0.1500 gram molybdenum, as ammonium molybdate, 40 cc. of sodium sulphide, and 120 cc. water were exposed to the influence of a current giving 0.7 cc. OH gas per minute. The time of deposition was twelve hours. The gold weighed 0.1430 gram.

(2) 0.1178 gram of gold, as chloride, 0.1200 gr. of molybdenum, 30 cc. of sodium sulphide (sp. gr. 1.12), and 100 cc. of water were electrolyzed with a current giving 2 cc. OH gas per minute.

The precipitated gold weighed 0.1172 gram. The current acted ten hours.

*Gold from Tungsten.*—(1) 0.1437 gram of metallic gold, as chloride, 0.1500 gram of tungsten, as ammonium tungstate, 20 cc. sodium sulphide (sp. gr. 1.17), and 100 cc. of water, gave 0.1440 gram gold, when electrolyzed with a current generating 1 cc. of OH gas per minute. The precipitation required twelve hours.

(2) In this trial 0.1440 gram of gold was found. The conditions were analogous to those of (1).

The gold deposits in all of the separations were bright and compact. The same manner of washing and drying was pursued as in the first determination of the metal.

Gold was never discovered in the liquid removed from the deposit.

Qualitative trials proved that the separation of gold from vanadium, under conditions similar to those already mentioned, was also possible.

CHEMICAL LABORATORY OF THE UNIV. OF PA.,

PHILADELPHIA, February 13, 1891. •

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PROCEEDINGS  
OF THE  
**ELECTRICAL SECTION,**  
OF THE  
FRANKLIN INSTITUTE.

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*[Proceedings of the first stated meeting of the Electrical Section, held Tuesday, February 24, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, February 24, 1891.

Prof. EDWIN J. HOUSTON, in the chair.

The stated meeting of the Section was held this evening, at 8 o'clock.

There were present: Messrs. Billberg, Davids, C. Hering, H. Hering Houston, Hoadley, McDevitt, Pemberton, Pike, Rondinella, Salom, Spangler Wahl, Winand, and fifteen visitors.

The minutes of the special meeting for organization, held Saturday, January 31st, were read and approved.

The following communications were presented:

By Mr. Wm. McDevitt—On some dangers of electric lighting. Discussed by Messrs. C. Hering, Houston, Billberg, Salom, and the author.

By Mr. C. Hering—On a new standard cell. Discussed by Messrs. Hoadley, Perkins, Pike, Houston, H. Hering, and the author.

By Mr. C. W. Pike—On a new accumulator. Discussed by Messrs. C. Hering, Salom, and the author.

By Prof. H. Hering—On the effect of atmospheric pressure on batteries. Discussed by Messrs. Pemberton, Houston, C. Hering, Wahl, Spangler, Pike, and the author.

On motion of Prof. Rondinella, the remaining papers on the programme were postponed until the next stated meeting, in order to permit of the transaction of routine business.

The consideration of the by-laws, provisionally adopted at the special meeting of January 31st, was first taken up. The secretary read the regulations, which were thereupon considered section by section, and after suffering a number of amendments, were adopted as a whole.



Nominations for officers were then made, as follows :

For president—Prof. Edwin J. Houston.

“ vice-presidents—Messrs. C. Hering and Pedro G. Salom.

“ secretary—Prof. L. F. Rondinella.

“ treasurer—Dr. Wm. H. Wahl.

“ conservator—Dr. Wm. H. Wahl.

The secretary *pro tem.*, was directed to cast the ballot of the meeting for the foregoing nominees, which accordingly was done, and the chairman *pro tem.*, declared them to be the officers of the section for the current year.

Mr. C. Hering moved a suspension of the rules, in order that the next stated meeting might be somewhat deferred. Carried.

The same member moved that the next stated meeting be held on Tuesday, March 10th. Carried.

The president thereupon named the following members to serve upon the committee on admission of members, viz: Messrs. C. Hering, H. Hering, Billberg, Pike, Salom, Rondinella and Wahl.

Adjourned.

WM. H. WAHL,

*Secretary pro tem.*

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[*Proceedings of the stated meeting, held March 10, 1891.*]

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, March 10, 1891.

Prof. EDWIN J. HOUSTON in the chair.

Present : Twenty-three members and visitors.

The minutes of the previous meeting were read and approved.

The treasurer reported that copies of the section's by-laws had been printed, and that the necessary stationery and supplies had been obtained for the secretary and treasurer.

The committee on admissions reported the following elections: To regular membership, Messrs. John Hoskin, T. Carpenter Smith, David Pepper, Jr., H. W. Bartol, F. J. Firth, Craig R. Arnold, Fred'k H. Colvin and Alex. J. Whittingham; and to associate membership, Messrs. D. S. Rittenhouse, Wm. McClellan and H. Allen Higgins.

Mr. R. W. Davids presented a paper on "A new form of megohm resistance."

There was discussion upon this standard and others of the same type by Messrs. Pike, Wahl, Houston, C. Hering, and Billberg. (Referred for publication.)

Dr. Wm. H. Wahl gave "A *résumé* of recent developments in the manufacture of aluminium," in which he briefly traced the improvements of the past five or six years in both metallurgical and electro-chemical methods, and instituted a comparison between these two types of processes with the view of estimating the possibilities of each in the future cheapening of the cost of producing the metal. His remarks were illustrated by lantern slides of the principal methods of manufactures, and were discussed at some length.

There was also considerable discussion respecting the recommendation by a committee of the American association for the advancement of science of the word "*aluminum*" for the name of the metal, Prof. Houston speaking strongly in favor of the retention of the name "*aluminium*," as being more in harmony with the nomenclature of the other metals, calcium, indium, magnesium, potassium, etc.

The meeting then adjourned.

L. F. RONDINELLA,  
*Secretary.*

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## A NOTE ON SOME DANGERS IN ELECTRIC LIGHTING.

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BY WM. McDEVITT.

Inspector of the Philadelphia Board of Fire Underwriters.

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[*Read at the stated meeting of the Electrical Section, held February 24, 1891.*]

After several years of experience in noting the dangers occurring in the use of electric lighting, it has been clearly demonstrated that in nearly every case the accidents resulted through ignorance both on the part of the workmen engaged in installing the wires and the lessee to whose care the new apparatus was entrusted.

These practical illustrations served as valuable lessons to those who witnessed them, more especially to the workmen, whose education in the business consisted merely in following plans laid out by the contractor.

Although the introduction of electric lighting has rapidly increased in our city, its use has been comparatively free from accidents, proving that most of the inherent dangers have been controlled, leaving but few features that require the attention of inventive minds to overcome. The most important of the existing defects is that relating to fusible connections, as the innumerable variety of alloys used for fuse metal is very misleading and is still an element of danger. Some of these compositions possess the properties of good electrical conductivity and are slow to heat; other compositions are of a character exhibiting weakness when heated, resulting in annoyance from continuous breaks and offering temptations (as has been found) to use ordinary wire in the absence of proper fuses.

Another universal danger in electric lighting results from the want of some uniform or more ready method of perfecting splices or joints in conductors. Some workmen are in the habit of making loose copper unions, leaving the solidity of the joint dependent on solder, which, being a metallic cement, is liable to be fused by a possible heavy short-circuit occurring on the line, thus melting the solder and leaving a loose connection.

Probably the most alarming danger exists through the possibility of lightning being conducted into houses lighted by electricity where the latter is supplied by aerial wires. The existence of this danger has been demonstrated where the house wires are attached to gas fixtures, offering a ready path for the lightning, which, in leaping from the charged wires to the gas pipes, carries the electric light current across, forming an arc, which pierces the pipe, and where gas is present, it will be ignited, causing a steady blaze. If this accident should occur near the ceiling, the building would be endangered.

With the rapid introduction of new appliances in electrical science, it may reasonably be expected that the defects above-mentioned will be overcome in the near future.

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## A NEW ACCUMULATOR PLATE.

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BY C. W. PIKE,

Instructor in Electrical Engineering, University of Pennsylvania.

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[*Read at the stated meeting of the Electrical Section, January 24, 1891.*]

While in Lowell, Mass., last year, a temporary craze upon the subject of storage batteries took almost complete possession of the town. The result of this was the production of several so-called new storage batteries. In reality, there was no attempt to produce a new type of accumulator, either by making use of new solutions or new metals. The sole novelty was in the form of the plates.

The inventors had but one object in view, namely, to construct a plate of such form that the active material would

not be dislodged from the cavities in which it was held, even by a severe shock or jar. Weight troubled them little; high discharge rate and large capacity less; and efficiency not at all. In spite of this, the results obtained by two of the inventors, Messrs. Bradbury and Stone, were not bad.

Two forms of plate were devised by these gentlemen, of which the first is very curious in its construction. Each plate consists of a number of elements, and each element is made by bending two strips of lead into what most of the



FIG. 1.

children, especially among the Germans, call a Jacob's ladder. (*Fig. 1.*)

This is then warmed and pulled in the direction of its length, taking care not to allow it to become twisted. When thus pulled out it has a number of interstices, into which active material can be placed. By placing several of these elements side by side and melting contiguous points together, a complete plate is formed, which is further strengthened by winding a lead strap around its perimeter, and melting points of the plate to it. On looking at the completed plate, it will be seen that every interstice on one side of the plate is connected to another on the opposite side of the plate, so that the active material, when put on, will consist of plugs of the shape seen in *Fig. 2*, so that the plug, as a whole, could not by any ordinary means be dislodged.



FIG. 2.

This form of plate has the advantage of large surface of active material and a fairly large ratio of weight of active material to weight of supporting plate, but the cost of making such a plate would more than overcome any advantages which it might have over ordinary forms of grid.

Realizing this, the inventors devised another form, made by bending lead strips over sticks of wood square in section, into the shape shown by *Fig. 3*. The first strip started from the top of the wooden stick, while the second strip started from the bottom of the stick; the third started from the top and

the fourth from the bottom, and so on. The plate, when completed, looked on either side like a checker-board: lead forming the black squares and the uncovered portions of the wooden strips the white squares. The strips were then



FIG. 3.

withdrawn and the plate strengthened, as in their first plate. This form of plate, though cheaper in construction than the former, was yet costly; so that they devised a mould, by which they were enabled to cast the plate entire. (*Fig. 4.*)

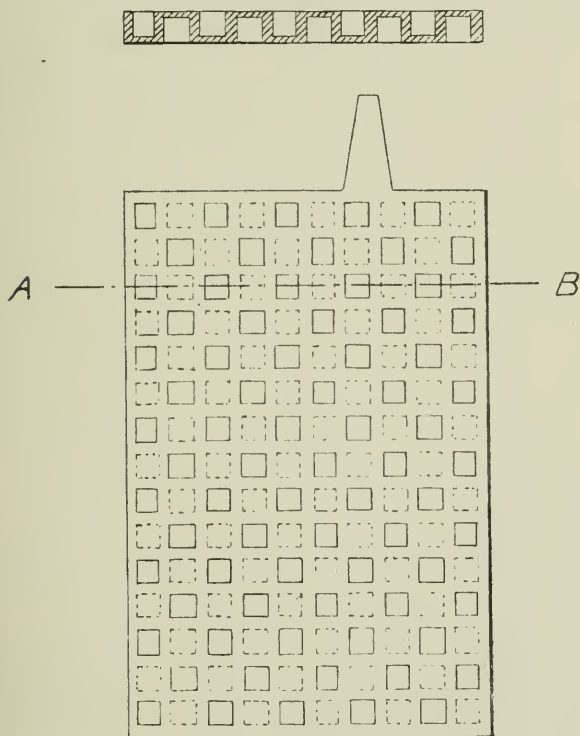


FIG. 4.

These plates before being filled are treated for a little while like a *Planté*, with a view to making the active material adhere strongly to the plate. The positives are



then filled with red lead, and the negatives with litharge. The active material can be put into the plates in three ways:

- (1) As a paste.
- (2) As a powder, forced in under pressure.
- (3) By pressing the oxides into sticks and driving these in.

I have suggested another method, namely, casting the plates around these sticks, virtually using them for a part of the mould.



FIG. 5.

Each plate has a lug cast on its top, and the plates of similar polarity are connected together by a lead strip cast to these lugs. (*Fig. 5.*) On the top of this connecting strip is also a lug, of the shape of a truncated cone, which serves to connect one cell to the next by means of a connector, of the shape shown in *Fig. 6.* This device is extremely simple and convenient, but, like all connectors, requires watchfulness in order to secure good contact.

On looking at this plate it seemed to me that it would accomplish very thoroughly the object for which it was designed, namely, to hold the active material in. Also that it would not buckle even when a very strong current was sent through it. Both of these opinions I have since found to be verified; the cells enduring an immense amount of jarring and short-circuiting without perceptible injury.

My impressions in regard to other points were less favorable. Of the total weight of a filled plate, but thirty-eight per cent. is active material; and of the total surface of the plate exposed to the electrolyte, but thirty per cent. is active material. Further, the plates are so thick that the inner portion of the active material will not be acted upon when the cell is rapidly discharged, since the outer portion

which has changed from lead peroxide to lead sulphate prevents fresh sulphuric acid from permeating as far as the peroxide inside with sufficient rapidity to keep up the electro-motive force of the cell.

These considerations led me to believe that the internal resistance would be rather high, and its storage capacity and efficiency low, unless a low rate of charge and discharge was used.

Until recently the only tests made on this cell were by a gentleman in Sioux City, who obtained from cells, of which the dimensions were 5 x 8 x 8 inches, and the weight thirty-four pounds each, a storage capacity of 100 ampère-hours and a "quantity efficiency" of seventy-six per cent. with a charging and discharging current of about ten



FIG. 6.

ampères. The real efficiency, or the energy taken out divided by energy put in, was not stated by him.

The cell which I received from Messrs. Bradbury and Stone, was the *worst* which they had on hand, having been short-circuited for hours at a time, and having been allowed to stand with the solution partially evaporated. Consequently the plates were quite badly sulphated, and I did not have sufficient time to get them in first-class condition.

The instruments used were a Weston voltmeter reading to five volts and a Weston ammeter reading to twenty-five ampères. The test for resistance of the battery, made by the current and potential galvanometer method, gave, as was expected, a rather high result, varying from '011 to '016 ohms.

The next test was to ascertain whether it would give a current in excess of its normal current for any length of

time. It was found that it would give twenty-five ampères for, at least, one hour. The next test was to determine the capacity and efficiency under different conditions.

The method of testing was as follows:

Discharge the cell at the desired rate till the P. D. was 1.8 volts. Immediately charge at the desired rate reading volts and ampères until charged. Note the duration of charge, and compute the ampère-hours  $AH$  and the watt-hours  $WH$ . As soon as charged, immediately discharge at the desired rate, reading as before; and compute ampère-hours  $A'H'$ , and watt-hours  $W'H'$ .

$$\frac{A'H'}{AH} = \text{quantity efficiency}$$

and

$$\frac{W'H'}{WH} = \text{real efficiency}$$

under the conditions of the test.

The result of three tests is as follows:

Average charging current.	Average discharging current.	Ampère-hours taken out.	Quantity efficiency, per cent.	Real efficiency, per cent.
12.07	15	65	61	51
11.2	11.2	56	77	65
10.02	10.03	75	75	66

They show, as with any cell, the gain in using low rates of charge and discharge; but beside this they show that this cell is very inefficient at a rate above ten ampères.

After the first test the cell was left open-circuited for twelve hours, after which twelve ampère-hours were taken out before the P. D. reached 1.8. After a further rest of three hours, six ampère-hours were taken out. This recovery, found in all cells, as was expected, is very much exaggerated in this cell on account of the thickness of the plates, and the cells would, in an installation where the work was intermittent, give results considerably better than is indicated by the table above. In all probability the capacity, efficiency and rate of discharge would be improved by making the plates thinner.

In the following table I have made a comparison of the different makes of cell as regards storage capacity, weight and volume. Not having weight and volume of the plates themselves at hand, I have been obliged to use the weight and dimensions of the entire cell. It will be seen that as regards the volume per ampère-hour, it compares favorably with the others, but in respect of ampère-hours per pound it is deficient.

	<i>Ampère-hours per pound of cell.</i>	<i>Cubic inches per ampère-hour.</i>
Accumulator, . . . . .	2'86	4'44
Bradbury-Stone, . . . . .	1'85	4'44
Detroit, . . . . .	2'65	4'63
E. P. S. { Stationary, . . . . .	1'91	7'06
{ Traction, . . . . .	2'26	5'53
Julien, . . . . .	2'81	4'33
Pumpelly, . . . . .	3'00	4'85

It is probable that a cell with this plate better designed and in better condition, would give results much more comparable to the others as regards efficiency, capacity and discharge rate, while it would surpass many of them in durability, and I hope at some future time to present to the section the results of some tests upon such a plate.

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## A NEW FORM OF MEGOHM RESISTANCE.

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BY RICHARD W. DAVIDS

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[*Read at the meeting of the Electrical Section, held March 10, 1891.*]

The instrument of which this paper treats is not new entirely, but has been used for some years in England to a limited extent, so that the title refers mainly to its introduction in this country.

It is designed to take the place, at a small cost, of expensive and cumbersome wire resistances having approximately equal values, or the use of varying shunts, different electromotive forces and the wire resistances in ordinary use in measuring commercially the insulation resistance of cables, air lines, etc., by means of proportional deflections on a

sensitive galvanometer. The resistance of these boxes varies from one to five megohms, although these limits are not necessary, so that they may be had of resistances approximating to those which are met in every-day use.

So far as it is known to the writer the construction is as follows: On an etched glass plate of some 4 x 6 inches a pencil line is drawn resembling a sinusoid; experience teaching to a small degree, however, its breadth and length. The pencil used is probably an ordinary lead, or rather graphite pencil, of a hardness found empirically to give the best results; this line is burnt into the plate most likely with the use of a flux; this operation must be done slowly, and takes some time. After a rough test for resistance to find if it is within the range sought, it is mounted in a block on springs to take up jars and make connection with the ordinary tall binding posts of apparatus needing high insulation.

There is no attempt made at adjusting these boxes: the plates are made and measured, any that are too high or too low being rejected, the surfaces being ground off and made over again. In this way a comparatively cheap instrument is obtained. They are not laboratory instruments, but designed to meet the want of every-day line and cable testing, where speed and not great accuracy is desirable.

A somewhat similar instrument for laboratory work consists of a plate of unglazed porcelain, or even a sheet of paper, having pencil marks made on it; connection being made by means of springs resting on these marks. The objection found in these forms is that a breath of wind or jar may so change the contacts of the graphite particles as to seriously alter the resistance.

Some twenty of these boxes were brought to the writer for calibration, and the question arose as to the best way to measure them accurately and quickly. The first method tried was with a Thomson reflecting galvanometer, finding its constant with the one-ninety-ninth shunt, and sufficient resistance to give nearly the same deflection as with the meg and no shunt, using in both cases forty cells of chloride of silver battery.



As checks, other measurements were made using no shunts, and varying the electro-motive force; and also the bridge method. It is fortunate that there were two boxes of 100,000 ohms each at hand, as the second and third terms of the proportion, for with these calibrated in terms of a standard box which was the first term, it was possible to determine accurately the meg, the fourth term.

It was disappointing to find that the checks did not check, the figures obtained by the shunt method being much out. As the shunts were found to be practically correct, it could not be due to them, and on repeating the measurement with accumulators, the discrepancy disappeared, showing that the silver battery, even with small currents, polarizes. Everything seemed to go nicely now, perfect faith being reposed in the bridge method, when, in order to get a closer balance, the battery was considerably increased, with the disheartening result that it appeared that the resistance depended on the current used. This change was very small—less than one per cent.—and negligible in practice, but it seems to show that with such small currents as  $\frac{1}{100000}$  of an ampère, appreciable change takes place, perhaps from heat, directly or indirectly.

It was an interesting point that the heat developed in such a box would raise one-sixteenth of a grain of water  $1^{\circ}$  C. in one minute. Supposing the resistance to be due to the imperfect contact of two adjacent molecules of carbon, the rise in temperature given could scarcely be enough to change the resistance.

It has been stated that change of temperature from  $0^{\circ}$  to  $40^{\circ}$  C. makes a difference of less than one per cent. in the value: also, that boxes made some years ago are practically the same now.

The use of the instrument was shown in one of the checks used. This was to find the comparative resistances of the various boxes by direct deflections, varying nothing but these with the megohms. With a very portable and accurate d'Arsonval galvanometer, the other boxes were checked rapidly; this was in effect measuring them, taking it for granted that any one was right.

In measurements on the street or in other places where it is difficult to set up and use an ordinary reflecting galvanometer, resistance boxes, different batteries, etc., all that would be necessary to get a very close measurement would be such a galvanometer, a suitable battery and such a megohm resistance.

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## BOOK NOTICES.

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*Lectures on the electro-magnet.* By Silvanus P. Thompson, D.Sc., B.A., F.R.A.S. The W. J. Johnston Company, limited, New York. 1891. 280 pp. 75 illustrations, 10 tables. Price, \$1.

No electrical book that has recently been published has been so welcomed and has been in such demand by electricians desiring practical directions for calculating and designing electro-magnets, as this most recent of the Johnston Company's publications.

Calculations concerning electro-magnets have been attended with much difficulty and considerable uncertainty until within recent years, owing to the lack of reliable data reduced to such shape that the average electrician can use it. Magnets for special purposes have usually been designed by means of repeated experiments and reconstructions; that course being preferable to very long, tedious and even uncertain calculation, which, in most cases, could not have been performed by the average designer of electro-magnets.

Researches in magnetism were deferred during the time when all attention was given to the production and application of electricity on a commercial scale, and to devising practical rules and methods of calculation. The lack of data concerning electro-magnets was severely felt, and it considerably hindered progress and the construction of efficient apparatus. Magnets for special purposes were usually guessed at and modified by proportion and experiment, and many were the dynamos and other apparatus using electro-magnets, whose failure or inefficiency was due to a lack of knowledge of the laws of magnetism.

A few years ago the current of investigation was, however, turned towards the principles of electro-magnets, in the desire to formulate the laws and furnish the much-needed data wherewith the work of magnets could be guided. Rowland's law of the magnetic circuit and Maxwell's law of traction were the bases of operation, and though much valuable information has appeared in the journals, no summary of a tolerably complete character had been published.

Prof. Thompson in his efforts to supply this great need, conducted a most thorough search through all the old and new English, French and German publications for records of reliable experiments and data about electro-magnets, and after collecting them and drawing his conclusions, added to them the result of numerous and original experiments of his own and pre-

sented them before the Society of Arts, London, in the form of four lectures, which constituted one of the sets of Cantor Lectures of the session, 1889-1890. These lectures were reprinted in the *Electrical World* during the past winter, and have now been issued in book-form, with some additions and revisions by the author.

With indefatigable labor, Prof. Thompson succeeded, by means of tables and simple formulæ, in reducing the calculations concerning electro-magnets to such practical shape, that the calculation of a magnet, to develop a definite number of lines of force, or to exert a pull of a definite number of pounds, is now a comparatively simple matter to a person possessing fair mathematical ability.

The first lecture contains, as is usual with Prof. Thompson, an "historical sketch" of electro-magnets, in which many of the famous magnets of the early days are described as well as many of the experiments made with them. The typical forms of magnets are briefly described next, and then comes a very clear and interesting discussion of the magnetic properties of iron and the methods of measuring permeability. A shop method of roughly estimating the permeability of iron by means of the "permeameter" devised by Prof. Thompson, is given and seems well suited to that purpose, as many of the objections to other methods are avoided. The lecture closes with a short and clear presentation of the phenomenon called "Hysteresis," and an interesting and instructive list of "Fallacies and facts about electro-magnets," many of which will astonish and surprise any one on first reading.

The second lecture is devoted chiefly to the general principles of design and construction. The principle of the magnetic circuit and the deduction of the formulæ is discussed in a very lucid way. The law of traction is developed and simple rules and formulæ with valuable tables and practical points are given, making the calculation of magnets for specific tractive force a comparatively simple matter. Then follows a discussion of the action of magnets at a distance from the armature, with and without pole pieces, and also the question of leakage and the reluctance of the air space between the poles; so that magnets for definite action at a distance can readily be approximated with considerable accuracy.

An appendix to the lecture contains a *résumé* of the calculations of the excitation and leakage of magnets, giving the rules and directions in a compact form.

The third lecture takes up the question of the winding of magnets. Many common-sense directions and suggestions are given and also a wire gauge and ampèreage table which will prove of great value to the designer of magnets. Unfortunately, however, it is calculated for the S. W. G. and not for the American gauge, and though the nearest B. & S. gauge is given, it is only a very rough approximation and will not do for accurate work. It is unfortunate that this table was not re-calculated and adapted to the B. & S. gauge throughout, as that would have made it far more valuable. A re-calculation of this table recently appeared in the *Electrical World*, but it is so full of mistakes that it is useless. The points given here about calculating the windings for magnets are very good and will prove very useful.

Next comes a discussion of special forms of electro-magnets; the effect of variation of the shape, size and position of coils, etc., are taken up and then the design of electro-magnets for special purposes, such as quickest action, longest range, etc.

Lecture four, is devoted to electro-magnetic mechanisms and gives the principles and practical directions for calculating magnets, whose cores or armatures are to have definite motions.

Altogether this little book contains a store of information of just the kind that is needed, and designers of magnets of all kinds will heartily welcome it, as one of the most useful and surely the cheapest of the electrical books ever published.

H. S. H.

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*History of the American piano-forte: Its technical development and the trade.* 12mo. By Daniel Spillane. New York: D. Spillane. 1890.

The author has succeeded in writing an interesting as well as instructive work. Beginning with a description of the spinet, harpsichord, virginal and other primitive keyed instruments, he proceeds to the invention of the piano-forte by Cristofori.

All the early piano-forte makers of Europe and America are duly mentioned, and their instruments described.

Much time and trouble have evidently been taken by the writer to get the accurate dates, particularly of the early American patterns.

The high repute of the pianos of Messrs. Steinway, Decker, Steck, Chickering, Knabe, Miller, Weber, and others, are justly substantiated.

This section is followed by a description of the kindred branches—action-making, key-making and the manufacture of implements used in the construction of a piano—which is, of course, indispensable in a work on this subject.

The whole is supplemented by a short history of musical journalism in America, from the founding of the *American musical journal*, in 1835, up to the present day, so rich in art journals, the press having always been closely identified with the piano trade. In fact, the subject is exhaustively treated, and nothing of real importance has been omitted. The work is dedicated to the Franklin Institute, in recognition of the influence of its early exhibitions in stimulating invention in this branch of the arts.

L. J. H.

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*Studies in statistics, social, political and medical.* By George B. Longstaff, M.A., M.B., etc. Royal. 8vo, with maps and diagrams. London: Edward Stanford. 1891.

This large and attractive volume gives evidence of much care and expense. The data are chiefly from the general registry office, London, but also from other sources inaccessible to most of the public. Among the valuable studies are: birth, death and marriage rates in England and Wales for fifty years; growth of population of England; migrations of peoples; growth of new nations (the United States, Canada, South America, South Africa and Australia). All of these four and others in the work are illustrated by hand-



some diagrams. As there are sixty-one pages devoted to the growth of the United States, this alone should recommend the work to Americans.

A chapter on increased population of modern cities, of course, brings in many statistics respecting London and other cities, especially their food supply, etc. There are many valuable data on the death rates, rise, continuation and decline of various diseases, while the causes and relative prevalence of some of them are examined. One notable subject admirably treated, and having numerous tabulations, is the recent decline in the English death-rate, considered in connection with the causes of death. It is to be regretted, however, that this article, excellent as far as it extends, is not made up to 1890, inclusive, but only to 1884.

The volume contains thirty maps and diagrams (most of them colored), intended to aid the various subjects; and suggestions for the census and tabular data of various diseases are not the least among its excellencies. The author may well be congratulated on his successful application of a "hobby" to public education in statistical matters. Evidently he is an enthusiast on figures and data; but only such persons can compile works of value in this department. As the "studies" discuss subjects interesting to insurance men, physicians, attorneys-at-law and students of economics in general, it is to be hoped that the author will receive due reward for this effort.

N.

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*Triple-expansion engines and engine trials.* By Prof. Osborne Reynolds. Edited by T. E. Idell, M.E. New York: D. Van Nostrand Company. 1890. pp. 191. 18mo.

This little book, which forms No. 99 of Van Nostrand's well-known "Science Series," is practically a reprint of Prof. Osborne's paper, read before the Institution of Civil Engineers, December 10, 1889, and published in vol. xcix of its proceedings, with an abstract of the discussion upon the paper.

It is exceedingly interesting, and places in a convenient form the methods and results of a series of observations which in themselves are well worth studying. The paper is a statement of the reasons for designing the engine of the Owens College laboratory in an unusual way, and contains a description of the engines and of numerous trials made with them. A short description of the engines may be interesting. The cylinders are 5 x 10, 8 x 10 and 12 x 15, and are arranged so that they can be worked as a triple-expansion condensing engine; as a compound condensing engine; or as a compound non-condensing engine; as a simple condensing, or as a simple non-condensing engine. Steam jackets can be used or not as desired. Large ports are provided and Meyer valves used, and the speed of the engines can be varied to 400 revolutions, and they can be worked with 200 pounds of steam. Each engine is arranged so that the work done by it can be absorbed in a Froude brake, ingeniously modified to fit the particular conditions. Each engine was allowed to run at the speed suited for using all the steam, and the cards were combined in such a way as to show the volume occupied by a given weight, say of one pound of steam in its passage through the cylinders. The latter half of the book is taken up with the discussion on



the paper, and about all the inaccuracies in the methods used are pointed out by Mr. E. A. Cowper, Prof. Unwin, Mr. W. W. Beaumont, Mr. Bodmer, Mr. Thorneycroft, Mr. Willans and others. The paper is well calculated to give the reader something to think about.

H. W. S.

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*Sewage disposal works.*—A guide to the construction of works for the prevention of the pollution by sewage of rivers and estuaries. By W. Santo Crimp. Philadelphia: J. B. Lippincott & Co. 1890.

The area of England is nearly the same as the area of the state of Georgia, while its population is nearly one-half that of the entire United States. Numerous commissions have been appointed, and numerous acts of parliament have been passed, for the purpose of preventing the contamination of the water supply of the cities of the kingdom by the sewage flowing from them. The volume before us presents the latest results obtained in the efforts made to improve the sanitary condition of this thickly populated country. Mr. Crimp's book is an octavo of over 250 pages, with numerous tabular statements, and is well illustrated by figures in the text, as well as by thirty-three lithographic plates drawn to scale. It is essentially a practical treatise, giving the actual working methods of sewage purification in thirty-three towns; a chapter being devoted to each town. Full particulars are given in each instance; such as the population of the given locality; daily flow of sewage; area of purification plant; methods of purification; cost of erecting plant; cost of working the process, as well as elaborate descriptions of the various methods of precipitating, settling and filtering the sewage. It may be of interest to state that of the various chemicals employed milk of lime, with or without the addition of a crude sulphate of alumina, seems to be the cheapest and most efficient, and is, therefore, most widely used.

This book should have a wide sale in this country. It treats of questions relating to public health, to which we shall soon have to give our careful attention. It will be particularly valuable to all sanitary engineers, and to municipal officials who have in their care the sanitation of our cities.

P.

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## Franklin Institute.

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[*Proceedings of the stated meeting, held Wednesday, March 18, 1891.*]

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, March 18, 1891.

Jos. M. WILSON, president, in the chair.

Present, 126 members and thirteen visitors.

Additions to membership since last report, twenty-two.

The actuary submitted the following extract from the minutes of the stated meeting of the board of managers, held March 11, 1891, viz:

"WHEREAS: The institute, at its stated meeting, held February 18, 1891, requested that the board of managers would express its opinion as to the advisability of the adoption of an amendment to article VI of the by-laws of the institute, offered by Mr. Wm. B. LeVan, viz: the addition thereto of a fourth section, to read as follows:

SECTION 4. No member shall be eligible for re-election to the office of manager, or be a candidate therefor, while he is filling such office, nor sooner than twelve months after the expiration of his term of office.

*Therefore, Resolved*, that the board of managers has considered the amendment proposed, and inasmuch as, of the thirty members of the board, twelve are nominated by the institute and elected annually, and as the amendment proposed would deprive the institute of the services of men who have faithfully served the institute for many years, without any corresponding advantage, the board would respectfully submit that in its opinion the adoption of the amendment would not be advisable."

Mr. William Kent, of New York, gave an oral description of the Springer torsion balance, illustrating his remarks by reference to a number of specimens of the apparatus, and with the aid of lantern views. (Referred for publication.)

Mr. John R. Jones read a paper describing his method and apparatus for rolling tapered axles. The paper was illustrated by means of lantern slides exhibiting the details of the machines for rolling the tapered forms and straightening the same after forming. (Referred for publication.)

Mr. S. Lloyd Wiegand described the Bacon machines for turning composite outlines in wood, to produce balusters, newel posts, and other ornamental forms for architectural uses, articles of furniture, etc. The work of these machines was shown by a large collection of specimens, exhibiting many complex and highly ornate forms. The machines are remarkable for the extraordinary rapidity with which they are able to operate, and for the smoothness of the work as it leaves the machine. A baluster or newel post of exceedingly complicated design is turned out in less than a minute.

Mr. Wiegand also described the Pentz boring and milling machines, and a universal joint used in conjunction therewith. This machine was recently reported upon by the Committee on Science and the Arts, and formed the subject of a recommendation of the John Scott legacy premium and medal.

Mr. J. Lynwood Garrison presented for inspection a suite of specimens of various articles fabricated from manganese-bronze, manufactured by B. H. Cramp & Co., of Philadelphia. These specimens illustrated, in an instructive way, the strength, malleability and other valuable qualities of this alloy. Mr. Garrison gave a short descriptive account of the history and various applications of this bronze.

The secretary's report embraced an account of the highly interesting and important discovery by Mr. M. Carey Lea, of Philadelphia, of certain allotropic modifications of metallic silver. A number of specimens were exhibited, which showed in a remarkable way the properties of the allotropic modifications of this metal, and their behavior towards various reagents.

Mr. Wm. B. LeVan's amendment to the by-laws was thereupon considered. The subject was discussed by Messrs. LeVan, Spangler, Fullerton and Lockwood. On motion of Prof. Spangler, action thereon was indefinitely postponed.

The secretary presented, on behalf of Prof. Edwin J. Houston, the following preamble and resolution :

"WHEREAS, Smoke from bituminous coal imperfectly burnt, in firing railroad locomotives, has been declared a public nuisance by three Grand Juries of Philadelphia County ;

*Therefore, be it Resolved*, that the Committee on Science and the Arts be requested to consider and report upon the best and most practicable way of abating or doing away with this nuisance, at the least inconvenience and loss to railroad companies firing locomotives with bituminous coal ; and to report what rules, regulations and penalties are enforced in London to prevent the emission of such smoke."

Mr. Garrison moved to amend the resolution to incorporate, as part of the subject of investigation, stationary as well as locomotive boilers. Carried.

Mr. Wiegand further amended the resolution to increase the scope of the investigation, so as to embrace the investigation of smoke prevention with fuels of all kinds used under boilers. Carried.

As so amended the resolution passed without dissent.

Mr. Wm. B. LeVan offered the following resolution :

"*Resolved*, That each paper intended for presentation and discussion at any meeting of this institute shall, if it pass the committee on meetings and the board of publications, be printed, with suitable illustrations, subject to revision, and be ready for delivery to each member, if so desired, at least one week prior to the meeting for which it is intended, and shall also be distributed at said meeting, at which it shall be read by title only. Discussion thereon shall then be in order ; discussions to be stenographically reported and appended to the revised copy of the paper, which revised copy, with the report of the discussion, shall constitute the authorized edition of the paper, and shall be part of the published transactions of the institute."

Referred to the committee on meetings with the request to report upon its advisability, or to suggest some practicable plan for accomplishing the object sought for.

On the secretary's motion, it was declared to be the sense of the meeting that the amendment to the by-laws creating the class of non-resident members, which was adopted at the stated meeting of February, 1891, should become operative on and after the beginning of the next fiscal year, to-wit : October 1, 1891.

Adjourned.

WM. H. WAHL, *Secretary*.

# JOURNAL

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THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the journal.

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## THE PROGRESS OF CHEMICAL THEORY: ITS HELPS AND HINDRANCES.

BY DR. PERSIFOR FRAZER, Prof. of Chemistry.

*[Introduction to the chemical course. A lecture delivered before the Franklin Institute, November 21, 1890.]*

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*[Continued from p. 252.]*

Hofmann (1660-1742), the first to distinguish between magnesia and alumina, asked whether instead of the above explanation of combustion it was not the case that a metal received an "acid" when burned, which "acid" was re-absorbed when the metal was reduced. Besides him many others had noted the increase of weight in combustion, and Boerhave (1668-1738) had doubted the explanation above referred to.

The offence of which as seekers after truth the phlogistonites were guilty was that of calling into existence a substance which was diametrically different in its properties

from any then known, and making this imaginary substance and its purely imaginary properties the basis of a theory.

Isaac Newton (1642-1727) had established, by his matchless investigations, commencing 1666, the nature and properties of matter, one of which, gravitation, was common to all bodies, and there was no excuse for an hypothesis after that date, which should deliberately rob matter of its one all-pervading attribute, unless some discovery had been made which seemed to support it.

If it be said that the observations on the combustion of bodies seemed themselves to furnish this proof, it must be admitted that for such an overthrow of all that had been patiently built up, some independent testimony unconnected with the then obscure phenomena of combustion ought to have been sought. This was an unpardonable deflection from the line of calm and dispassionate inquiry, and deserves to be held up for all time to the condemnation of scientific men as a warning, and its fate as an example; the more so because as time went on and the array of obstacles to the acceptance of this theory increased, its supporters were obliged to set up one after another distinct hypotheses to support the first untenable one, until the discussion partook more of the nature of those verbal juggles popular among the schoolmen of the middle ages than the efforts of students of nature to arrive at a knowledge of her laws.

Marggraf (1709-1783) introduced into chemistry the study of reactions in the wet way, and thus laid the foundation of analytical chemistry. He recognized soda as an alkali, and magnesia and alumina as peculiar earths. Against the opinions of his predecessors he held that ammonia was not produced in the distillation of wood, etc., by the union of its constituents, but that it pre-existed in the wood.

J. Black, of Scotland, by his investigation of the alkalis, dealt the first serious blow to the phlogistic theory. He found that magnesia could be changed to "mild" from having been strongly alkaline by exposure to air, or by contact with "mild" alkalis. In other words, that it became carbonate of magnesia by exposure to the carbonic acid of the air or by treatment with the carbonates of the alkalis.



Furthermore, he showed that what it lost or gained in these changes was a gas like air, which separated from it under treatment by acids with effervescence, whereas in its caustic condition acids dissolved it without effervescence. He also, with singular astuteness, established for his theory of combustion (which was to finally take the place of the phlogistic theory) the principle of latent heat. The immediate result of Black's discoveries was the rise of pneumatic chemistry or the chemistry of the gases.

Joseph Priestley (1733-1804) followed with more success than any other this line of investigation. He worked sporadically and without system, but with wonderful penetration discovered many things that had escaped the attention of better chemists. He himself called his achievements "sportsman's luck." He found the nitrogen left after combustion in air, and determined its proportion by volume. He regarded it as charged with phlogiston.

In 1774, he discovered oxygen in the oxide of mercury, and recognizing it as the supporter of combustion in the air, made it the measure of the destruction or devitalization of the air. He discovered besides oxygen and nitrogen, the reduction of the calxes by hydrogen. He also observed that by the passage of the electric spark through confined air in contact with moistened litmus, a new acid was produced which colored the latter red. He furnished more material than any other chemist of the day for the destruction of the false phlogistic theory, but he was not only incapable of correctly reasoning on the facts which he had brought to light, but to the day of his death he maintained that his pivotal discovery; the corner-stone of the chemistry of to-day (as it was made by Lavoisier) was nothing but the production of dephlogisticated air.

It was a touching thought for those of us who, in 1874, assembled at his grave in Northumberland, Pa., to do honor to his memory as a great discoverer and a devoted friend of our young States when we threw off the English yoke, and to celebrate the centennial anniversary of the foundation of modern chemistry, in his isolation of oxygen; that this great mind so potent in dis-

covery in the science which he adorned, rejected the obvious fruits of that discovery, and insisted on rejecting the honor which should justly have been his.

H. Cavendish (1731-1810) was a more rigid investigator, who, having discovered hydrogen by the action of sulphuric or hydrochloric acid on iron or zinc, proceeded to examine exhaustively the properties of this new gas. He noted that like weights of metal gave him the same volumes, but that different weights of metals gave him different volumes of the gas. He concluded that hydrogen was either phlogiston or a combination of phlogiston with water, and that dephlogisticated air (oxygen) when combined with phlogiston (hydrogen) produces water.

He discovered the solubility of carbonates of the alkaline earths in excess of carbonic acid water, and discovered nitric acid. He was thoroughly saturated with the phlogistic theory and all the useful deductions which his careful methods would have given him were thrown away by the distortions of the obvious bearing of his experiments which the phlogistic theory necessitated. When Lavoisier had triumphantly overthrown this dragon, Cavendish abandoned chemistry; simply remarking that it was hard to determine which of the rival theories was true.

Passing over the labors of the phlogistonists: Scheele, who discovered chlorine and fluo-silicic acid, and who believed light and heat to be fire and air; the first with more and the second with less phlogiston; and Bergmann, who systematized wet analysis, proved the existence of carbonic acid in the air, and introduced dry qualitative blowpipe analysis; we come to the first of a number of great figures in the history of modern chemical theory whose mission it was to open the door of the exact science of to-day and to indicate the route of those who followed him.

Lavoisier (1743-1794). His first chemical work was the experimental proof that water did not become an earth by boiling, but that the residue observed by boiling in a glass vessel was derived from the glass. His own discoveries are meagre in comparison with those of many of his con-

temporaries; but he greedily absorbed all that was discovered by others and changed it from crude and disconnected facts into an harmonious and consistent system.

All of his contributions to chemical science were of this character, even that great conception of the difference between the least distinguishable part of a certain kind of matter, and the least part which can take part in chemical reactions—the germ of the future distinction of the then unknown atom and molecule.

The last entrenchment of the phlogistonites was in the observed action of acids on certain metals whereby hydrogen (phlogiston) was produced. "If," they said, "a metal be not a combination of a calx with phlogiston, whence comes the phlogiston produced by the experiment?" The obvious answer was that it came from the acid, but what part? The acid was thought to unite with the metal and dephlogisticate it. Priestley and Cavendish had shown that hydrogen (phlogiston) and dephlogisticated air combined and in great part disappear. What became of them was not satisfactorily settled for a quite surprisingly long time. The fact that a little moisture was observed in the apparatus was probably ascribed to the sudden expansion and contraction of volume of the gases; since these matters and the hygroscopicity of the air were only beginning to be understood. When, therefore, Cavendish announced his discovery of the composition of water, Lavoisier applied it to the theory of combustion with such telling effect that it once and for all overthrew and destroyed phlogiston.

But though this was a glorious service of Lavoisier, it was far from all that chemistry owes to him. The new conceptions required a new language to express them, and Lavoisier with Guyton de Morveau established a system and a nomenclature so perfect as to form the frame work enclosing within itself all systems which followed.

A series of definitions and rules for naming new combinations as well as a partial list of elements were parts of this system.

Lavoisier came back to Robert Boyle's definition of an element as a body which cannot be decomposed into simpler

ones. The table of these elements, published by the French colleagues, contains also heat and light, but this was not in conformity with the opinion of Lavoisier, who was far too careful a student of nature to commit himself to any such gratuitous assumption; but rather more probably a concession made to the defeated phlogistonists and a means of avoiding the necessity of explaining that about which the views of scientific men were very conflicting, and nothing was certainly known.

One generalization Lavoisier allowed himself, and that was that all acids contain oxygen (hence the name he gave to that element). It is curious how fate punishes such generalizations whenever they are made in the infancy of our knowledge on any subject. This generalization reflects great credit upon its proposer, and shows a rapid and profound insight into many facts, but it was not many years afterwards pronounced, and is now generally considered, a fallacy, and yet if we interpret it to mean the exhibition of acid characters such as the reddening of litmus and the effect upon the senses, Lavoisier was not entirely wrong; since these characters are inseparable from the presence of water which contains oxygen.

The long chemical war against phlogiston had been fought and won, and the thought of experimenters was turning in a new direction which was to institute a new war lasting only a little less long than the last, but the difference between the two cases was that whereas the phlogistic theory hung like a pall over the whole science, obscuring during its continuance the entire field; in this case the question in dispute was as to the ultimate constituents of matter and none of the many views entertained on these questions interfered with the classification and assimilation of the myriads of facts which experiment and research were eliciting. This war, therefore, while it will serve to illustrate that the most eminent chemists share with the rest of the world the weaknesses of our common humanity, did not materially retard the progress of the theory of chemistry.

Proust (1755-1826) announced the unchangeable propor-



tions by weight in which substances combine together; and that if they combine in more than one proportion it is by leaps and not gradually as the water of the ocean becomes little by little more charged with salts brought down to it by the rivers. This was a great and pregnant discovery which at once led the way to the new field of battle, but the strangest thing about this announcement is that it was vehemently attacked by Berthollet (like Proust, a native of France) in a proposition which a little later seemed nothing but a stupid blunder or obstinate opposition, and yet in Berthollet's contention lay a precious truth only recently recognized and placed in its proper place.

Briefly the skirmish between these two men was this: Proust discovered that the relative proportion to each other by weight of carbonic acid and copper in carbonate of copper was constant, no matter in how great excess one or the other of these bodies was present, the weight of the carbonate of copper was the same and the weight of each constituent in it was invariable. For instance, substituting the accurate weights which better methods and apparatus have enabled chemists to obtain for the inaccurate approximations then made, in 123.4 grams of carbonate of copper, (there were always 63.4 grams of copper and 60 grams of carbonic acid. It made no difference whether these weights of the two elements respectively, or whether two or three times as much of one with the above weight of the other were made to combine: the result was always that 123.4 grams of the compound were found and the excess of either element remained uncombined.

With tin and iron there were two proportions by weight in which each of these elements combined with oxygen, but there were no insensible passages from one to the other. Thus there was a compound of oxygen and tin in which 119 weight-units of the latter combined with 16 of the former; and there was another in which 119 tin combined with 32 of oxygen but there was none in which the 119 weight-units of tin combined with any number of weight-units of oxygen between sixteen and thirty-one.



Berthollet (1748-1822) maintained, on the contrary, that if different masses of two elements are brought together, there will be found in the compound more of that constituent which was in greater quantity before the union. On account of the high position which Berthollet held in the chemical world this view received respectful, though silent attention, for few of the masters of the science were won over by it; because Richter, Klaproth, Vauquelin, and Wenzel had placed the constancy of acid and base in a compound of the two, beyond all question. Proust, however, took up the gauntlet and followed each separate publication of Berthollet by a refutation based upon careful experiment. This lasted for eight years, or from 1799 to 1807, and was settled apparently forever when Proust, by repeating some of Berthollet's own experiments on the successive stages of oxidation showed that his opponent had mistaken a percentage of water for a percentage of oxygen.

But Berthollet's main idea that the mass and the affinity were inseparable factors in the formation of a compound, after having been crushed to earth was to rise again in more recent times by the labors of his countryman of almost similar name, Berthelot and St. Gilles, and by Guldberg and Waage; but they showed, not that the proportion by weight of the compound, but that the rapidity of the reaction was affected by the masses of the constituents.

This dispute and the rise and fall of a theory was only a slight skirmish, which was preliminary to the general engagement. It had an admirable effect on the science, widened men's views, proved that the weapon of the future was to be carefully conducted experiment; and without doubt ripened the next great discovery which was then about to be announced.

John Dalton (1766-1844) was led to the happy thought of taking the data of the weights which Proust had announced as those in which tin, iron, oxygen, etc., combined, and reducing them to their simplest proportions. Proust had found that some arbitrary number of grams of tin (say, for example, 119) combined either with 16 or with 32 grams

of oxygen, and with no other weights. Dalton showed that the weights of oxygen in these two compounds were to each other as 1 to 2.

In the same way the different weights of sulphur which entered into combination with a given weight of iron were to each other as 1 to 2. And he found that this held for all cases where two constituents combined with each other in more than one proportion.

Thus, if the amount of hydrogen in olefiant gas or ethylene and marsh gas or methane are compared they are to each other as 1 to 2. By numerous examinations of this kind, in all of which he found this simple relation, he was led to formulate his atomic theory, some of the more important propositions of which may be thus condensed. (1) Every element consists of similar atoms of fixed weight; (2) Chemical combinations are made by the union of the atoms in the simplest proportions. The atomic weight of a compound is equal to the sum of the atomic weights of its constituents. He supposed all the atoms to be spherical and to be surrounded by heat spheres (!)

It should be mentioned in passing that Higgins had said in 1789 that chemical smallest particles were united to form compounds in simplest proportions, but as he never adduced any proof of this, the merit of the discovery belongs to Dalton by the law of possession already alluded to before, viz: that in natural science not only must a truth be announced, but some reason for it must be given.

The immediate result of his postulates was that Dalton set out to establish a scale of atomic weights for the elements. Among minor postulates of his which have not lived till our day, but which were natural enough at a time when there were no means of obtaining certainty as to the questions of the number of atoms entering into combination, etc., was this, that if only one proportion by weight of a combination between two elements were known, it must be supposed that the number of atoms entering into this combination was one from each element. If two were known, then that in which the least weight of one combined with the least weight of the other must be considered 1 to 1;

when with double this weight of the other the proportion must be 1 to 2, etc. In Dalton's time only one combination between oxygen and hydrogen was known, viz : water, and he assumed this to be composed of one atom of H to one atom of O. As H was and is yet the lightest element known he assumed its weight as one. By the imperfect methods then available, he determined the weight of O which combined with it to form water as 6.5 (in reality it is 7.98 if  $H = 1$ ).

Ammonia, which was the only compound of H and N known to him, and in which he also assumed one atom of each element, gave him the number 5 for the atomic weight of N. By accurate methods it should be 4.66.) In the lowest compound of carbon and oxygen known, carbonous oxide, he found the atomic weight of carbon 5.4 calling oxygen 6.5 (the right figure is 6).

All his figures were wrong as we now believe because of his false assumption of the constitution of water (not to speak of his imperfect methods of analysis), yet the accuracy which he attained was surprising for his epoch and the invigorating effect on the science was as great as if all his numbers had been absolutely correct.

Humphry Davy (1778-1829), the great discoverer of the alkaline metals and earths, who first announced the elemental character of chlorine, and by his discovery of the halogen acids seemed to have overthrown Lavoisier's dictum regarding the invariable presence of O in all acids : Davy, the discoverer of the safety lamp for miners, first announced his belief that chemical affinity and electricity were the same force. This idea was erected by Berzelius later into the splendid structure which he called the electro-chemical theory. Neither Davy nor Wollaston believed that Dalton's experiments had succeeded in establishing the nature and characters of atoms, but contented themselves with Wollaston's theory of "equivalents," without seeking to define how much matter entered into combination.

Their theory was that the atomic weights of Dalton were merely a series of arbitrary numbers, showing the respective quantities of different elements which were equivalent to each other in combining each with a third.

Wollaston's name of "equivalents" took root later after the apparent failure of Berzelius' theory to account for all the facts, and was the shibboleth of a long period of timidity and vacillation in chemical theory, which marked the reaction of thought when it was feared that the allurements of a beautiful system and the powerful influence of a great authority had drawn the representatives of the science away from sure ground. This period of intellectual cowardice was very tantalizing and very confusing to those who pursued their studies during this period, but in the end it was an advantage to the science by letting the field lie fallow for a time, and making it thus the fitter to receive and develop the seed which finally was sown upon it.

In all cases where the development of a science has been rapid, it is found that the great minds are clustered together, and that the great discoveries occur in succession to supplement each other. It was stated that the discovery by Black originated pneumatic chemistry or the chemistry of the gases. In this field the discoveries of Cavendish, Priestley and Scheele were made, but with the wider view given by Lavoisier to the science, the study of the gases was abandoned for the study of other solid and liquid compounds. But Gay-Lussac (1778-1850) devoted himself to pneumatic chemistry and accomplished in it what supplemented the work of Dalton and prepared the way and assisted the researches of Berzelius.

In 1805, in conjunction with Alex. v. Humboldt, Gay-Lussac established the fact that exactly two volumes of H combine with one volume of O to form water.

He showed the simple relations of the volumes of combining gases to each other and to their compound: he showed the effects of temperature on gases, and how it must be considered in connection with the Boyle-Marriotte law of pressure. His conclusion was that "The specific gravities of gases are proportional to their atomic weights, or are simple multiples of them."

Avogadro, an Italian chemist and physicist, attracted by the discoveries of Gay-Lussac, had, in 1811, deduced from the Boyle-Marriotte law that in equal volumes of two gases at



the same pressure and temperature must be contained an equal number of physical particles. It was such a small step from these two beautiful generalizations to the conclusion that the smallest physical parts of elementary gases not being indissoluble must contain more than one atom, and that therefore here was proven the physical smallest parts, and the still smaller chemical smallest parts which are capable of entering into combination. But though Avogadro announced this conclusion in 1811, it was long years before it was taken up and embodied in the theory of the science. Gay-Lussac furthermore, by his work on iodine and cyanogen, laid the foundation of the "radicle;" as his experiments on the action of chlorine on oils did the same for the "substitution theory." He is also the inventor of the method of volume analysis or titrimetry.

J. J. Berzelius (1779-1848) offers a life history such as few have been seen since the beginning of the world. It seemed as if the tangled skeins of nature's most intricate clues were straightened and cleared in his hand as if by magic; and the marvel of the magnificent work which he left as a monument to himself, his country and the science to whose cause he was devoted, is that hardly a line of it needs to be erased, and the corrections of his constants are decimals representing the greater accuracy of apparatus at the present day.

In 1812 he created a new mineral system, in which the combination of the elements in multiple proportion was clearly indicated. His improved methods of analysis served to enable him in 1814 to show that also in organic chemistry this law prevailed. The atomic theory was made by him the guiding principle for the science. He explained the union of elements by the polarities peculiar to their atoms and his electro-chemical theory founded upon this hypothesis brought him to the dualistic view of the combination of matter.

The reason why so little of his work needs to be changed is that he based everything upon investigation and experiment. The results which he achieved here will remain, no matter what theory may be the final outcome of further



advance. He saw at once that Dalton's rule for determining the relative number of atoms in a compound was arbitrary and he pronounced it so. With the help of Gay-Lussac's discoveries of the gas volume relations and his own discovery of the oxygen law of relation between the acid and the base, he was enabled to draw correct conclusions as to at least 2,000 bodies which he had personally analysed. He considered the unit volume represented by the atom, and he deduced the constitution of bodies by weight and by volume (as, for instance, water) as we understand them to-day. It is only fair to observe that Berzelius himself, in spite of his strong belief in the power of the volume theory to assist the investigator to a knowledge of the true atomic relations of a chemical element, recognized its limitations and rejected altogether the efforts to apply it to bodies which could not be studied in the gaseous state.

This is only just to bear in mind, because an onslaught against the splendid structure which his skilful hands had erected was caused by a mistaken notion as to Berzelius' real views of the atom volumes. This onslaught caused the paralysis of the faculty of speculation for many years among chemists, converting one of the most enchanting and exciting fields of discovery into the mechanical record of dry facts, which it was contrary to the fashion of the day to seek to unite under any common cause. Had Berzelius' teachings been properly understood and heeded, there had been no occasion for this panic, and the last days of the grand old Pioneer would not have been embittered by the thought that the labor of his life, which was good work, was doomed to destruction.

The atom values which he had obtained in 1818 are given herewith.

	<i>Berzelius.</i>	<i>Correct.</i>
C, . . . . .	12'12	12'
O, . . . . .	16'0	16
S, . . . . .	32'3	32'06
Pb, . . . . .	416'0	206'95
Hg, . . . . .	406'0	200'00
Cu, . . . . .	129'0	63'4
Fe, . . . . .	109'1	56'0

	Berzelius.	Correct.
Na, . . . . .	93'5	23'05
Ka, . . . . .	157'6	39'11
Ag, . . . . .	433'7	107'92

The reason that the values he obtained for the metals were so much higher than ours now, was that he then doubted the occurrence of other oxides than  $\text{MO}$ ,  $\text{MO}_2$ ,  $\text{MO}_3$ , etc.,  $\text{M}$  standing for the metal. Instead of  $\text{FeO}$ ,  $\text{Fe}_2\text{O}_3$ , he wrote these compounds  $\text{FeO}_2$ ,  $\text{FeO}_3$ , and consequently his percentage of iron was doubled. For similar reasons the metals  $\text{K}$  and  $\text{Na}$  received four times their normal weight since he regarded the compound which we know now as  $\text{K}_2\text{O}_4$  as  $\text{KO}_3$ . Some years later he modified this position, admitting the existence of  $\text{M}_2\text{O}_3$ , and his table then conforms nearly to the present. In his further classic work of determining the atom weights, he was assisted by the beautiful discovery of Dulong and Petit in 1819, that the atoms of all elements have the same capacity for heat, or that the product of the specific heat into the atomic weight gives a constant quantity.

Mitscherlich, a student of Berzelius, discovered that compounds of different elements which were similar in the number of atoms, the equivalent of water, etc., were isomorphic in crystallization.

[To be continued.]

## RIVETED JOINTS IN BOILER SHELLS.

BY WILLIAM BARNET LE VAN.

[Read at the stated meeting of the Institute, held November 19, 1890.]

[Concluded from p. 267.]

*Fig. 11* represents the spacing of rivets composed of steel plates three-eighths inch thick, averaging 58,000 pounds tensile strength on boiler fifty-four inches diameter, secured by iron rivets seven-eighths-inch diameter. Joints of these dimensions have been in constant use for the last fourteen years, carrying 100 pounds per square inch.

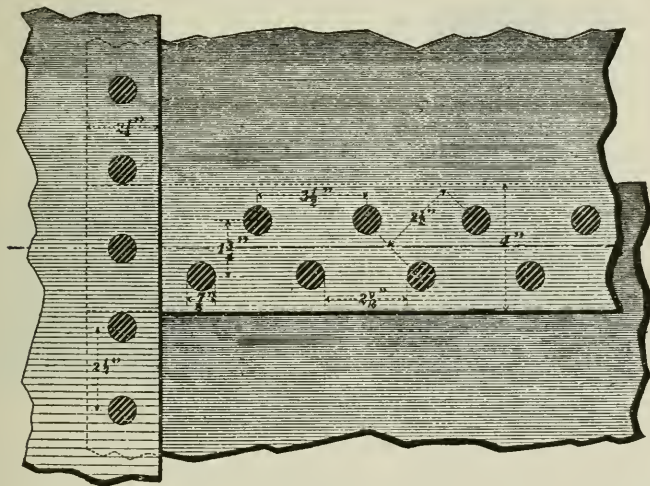


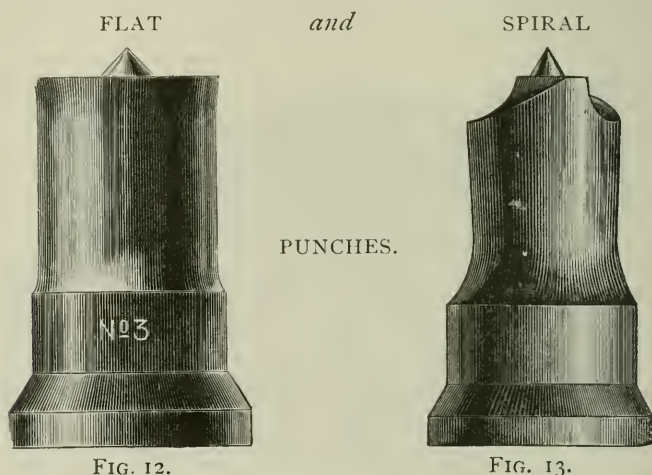
FIG. 11.

*Punching rivet holes.*—Of all tools that take part in the construction of boilers none are more important, or have more to do, than the machine for punching rivet holes.

That punching—or the forcible detrusion of a circular piece of metal to form a rivet hole—has a more or less injurious effect upon the metal plates surrounding the hole, is a fact well known and admitted by every engineer, and it has often been said that the rivet holes ought all to be drilled. But, unfortunately, at present writing, no drilling appli-

ances have not yet been placed on the market that can at all compare with punching apparatus in rapidity and cheapness of working. A first-class punching machine will make from forty to fifty holes per minute in a thick steel plate. Where is the drilling machine that will approach that with a single drill?

The most important matter in punching plates is the diameter of the opening in the bolster or die relatively to that of the punch; this difference exercises an important influence in respect not only of easy punching but also in its effect upon the plate punched. If we attempt to punch a perfectly cylindrical hole, the opening in the die block



must be of the same diameter as the point of the punch, or, at least, a very close fit. The point of the punch ought to be slightly larger in diameter than the neck, or upper part, as shown in *Figs. 12* and *13*, so as to clear itself easily.

When the hole in the bolster or die block is of a larger diameter than the punch, the piece of metal thrust out is of larger diameter on the bottom side, and it comes out with an ease proportionate to the difference between the lower and upper diameters; or, in other words, it produces a taper hole in the plate, but allows the punching to be done with less consumption of power and, it is said, with less strain on the plate.

As to the difference which should exist between the diameter of the punch and die hole, this varies a little with the thickness of the plate punched, or should do so in all carefully executed work, for it is easy to understand that the die which might give a suitable taper in a three-fourths-inch plate would give too great a taper in a three-eighths-inch plate. There is no fixed rule; practical experience determines this in a rough-and-ready way—often a very rough way, indeed, for if a machine has to punch different thicknesses of plate for the same size of rivets, the workman will seldom take the trouble to change the die with every variation of thickness. The maker of punches and dies generally allows about three-sixty-fourths or 0.0468 of an inch clearance.

The following formula is also used by punch and die makers:

$$\text{Clearance} = D = d + 0.2t$$

where

$D$  = diameter of hole in die block;

$d$  = diameter of cutting edge of punch;

$t$  = thickness of plate in fractions of an inch;

that is to say, the diameter of the die hole equals diameter of punch plus two-tenths the thickness of the plate to be punched.

*Example.*—Given a plate  $\frac{3}{8}$  or 0.375 of an inch thick, the diameter of the punch being  $1\frac{3}{8}$  or 0.8125 of an inch, then the diameter of the die hole will be as follows:

$$\text{Diameter of die hole} = 0.8125 + 0.375 \times 0.2 = 0.8875$$

inch diameter,

or say  $\frac{7}{8}$  or 0.875 inch diameter.

Punches are generally made flat on their cutting edge, as shown in *Fig. 12*. There are also punches made spiral on their cutting edge, as shown in *Fig. 13*. This punch instead of being flat, as in *Fig. 12*, is of a helical form, as shown in *Fig. 13*, so as to have a gradual shearing action commencing at the centre and travelling round to the circumference. Its form may be explained by imagining the upper cutter of a



shearing machine being rolled upon itself so as to form a cylinder of which its long edge is the axis. The die being quite flat, it follows that the shearing action proceeds from the centre to the circumference, just as in a shearing machine, it travels from the deeper to the shallower end of the upper cutter. The latter is not recommended for use in metal of a thickness greater than the diameter of the punch, and is best adapted for thicknesses of metal two-thirds the diameter of the punch.

*Fig. 14* shows positions of punch and attachments in the machine.

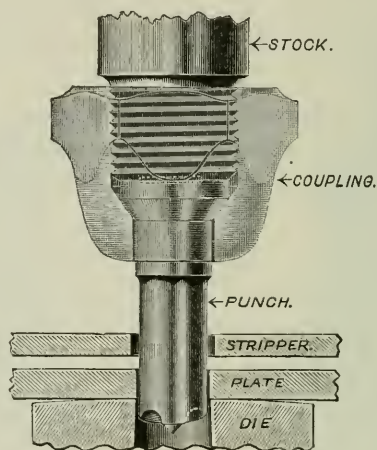


FIG. 14.

It is of the greatest importance that the punch should be kept sharp and the die in good order. If the punch is allowed to become dull, it will produce a fin on the edge of the rivet hole, which, if not removed, will cut into the rivet head and destroy the fillet by cutting into the head. When the punch is in good condition it will leave a sharp edge, which, if not removed, will also destroy the fillet under the head by cutting it away.

Punching possesses so many advantages over drilling as to render it extremely important that the operation should be reduced to a system so as to be as harmless as possible to the plate. In fact, no plate should be used in the construction of a boiler that does not improve with punching,

and further on I will show by the experiments made by Hoopes & Townsend, of Philadelphia, that good material is improved by punching; that is to say, with properly made punches and dies, by the upsetting around the punched hole, the value of the plate is increased instead of diminished, the flow of particles from the hole into the surrounding parts causing stiffening and strengthening.

*Drilling rivet holes.*—In the foregoing I have not referred to the drilling of rivet holes in place of punching. The great objection to drilling rivet holes is the expense, from the fact that it takes more time, and when drilled of full rivet size we are met with the difficulty of getting the rivet holes to correspond, as they are when punched of full-rivet diameter. When two plates are drilled in place together, the drill will produce a *burr* between the two plates—on account of their uneven surfaces—which prevents them being brought together, so as to be water and steam tight, unless the plates are afterwards separated and the burr removed, which of course adds greatly to the expense.

The difference in strength between boiler plates punched or drilled of full rivet size may be either greater or less than the difference in strength between unperforated plates of equal areas of fracture section. When the metal plates are very soft and ductile, the operation of punching does no appreciable injury. Prof. Thurston says he has sometimes found it actually productive of increased strength; the flow of particles from the rivet hole into the surrounding parts causing stiffening and strengthening. With most steel and hard iron plates the effect of punching is often to produce serious weakening and a tendency to crack, which in some cases has resulted seriously. With first-class steel or iron plates, punching is perfectly allowable, and the cost is twenty-five per cent. less than drilling; in fact, none but first-class metal plates should be used in the construction of steam boilers.

In the original punching machines the die was made much larger than the punch, and the result was a conical taper hole to receive the rivet. With the advanced state of the arts the punch and die are accurately fitted; that is to

say, the ordinary clearance for a rivet of (say) three-fourths inch diameter, the dies have about three-sixty-fourths of an inch, the punch being made of full rivet size, and the clearance allowed in the diameter of the die.

Take, for example, cold-punched nuts. Those made by Messrs. Hoopes & Townsend, Philadelphia, when taken as specimens of "commercial," as distinguished from merely experimental, punching, are of considerable interest in this connection, owing to the entire absence of the conical holes above-mentioned.

When the holes are punched by machines properly built, with the punch accurately fitted to the die, the effect is that the metal is made to flow around the punch and thus is made more dense and stronger. That some such action takes place seems probable, from the appearance of the holes in the Hoopes & Townsend nuts, which are straight and almost as smooth as though they were drilled.

Therefore, I repeat, that iron or steel that is not improved by proper punching machinery, is not of fit quality to enter into the construction of steam boilers.

#### STRENGTH OF PUNCHED AND DRILLED IRON BARS.

HOOPES & TOWNSEND.			
<i>Thickness of bar in inches.</i>	<i>Thickness outside of hole in inches.</i>	<i>Punched bars broke in pounds.</i>	<i>Drilled bars broke in pounds.</i>
$\frac{3}{8}$ or 0.375	$\frac{3}{8}$ or 0.375	31,740	28,000
$\frac{3}{8}$ or 0.375	$\frac{3}{8}$ or 0.375	31,380	26,950
$\frac{5}{8}$ or 0.625	$\frac{1}{4}$ or 0.25	18,820	18,000
$\frac{5}{8}$ or 0.625	$\frac{1}{4}$ or 0.25	18,750	17,590
$\frac{5}{8}$ or 0.625	$\frac{3}{16}$ or 0.1875	14,590	13,230
$\frac{5}{8}$ or 0.625	$\frac{3}{16}$ or 0.1875	15,420	13,750
$\frac{5}{8}$ or 0.625	$\frac{1}{8}$ or 0.125	10,670	9,320
$\frac{5}{8}$ or 0.625	$\frac{1}{8}$ or 0.125	11,730	9,580

It will be seen from the above, that the punched bars had the greatest strength, indicating that punching had the effect of strengthening instead of weakening the metal. These experiments have given results just the reverse of similar experiments made on boiler plates; but the material, such as above experimented upon, is what should be placed in boilers, tough and ductile, and the manner of, and care taken in, punching, contribute to these results.

It is usual to have the rivet holes one-sixteenth of an inch in diameter larger than the rivets, in order to allow for their expansion when hot; it is evident, however, that the difference between the diameters of the rivet hole and of the rivet should vary with the size of the rivet.

The hole in the die is made larger than the punch; for ordinary work the proportion of their respective diameters varies from 1 : 1.5 to 1 : 2.

As I have before stated, the best plate joint is that in which the strength of the plate and the resistance of the rivet to shearing are equal to each other.

In boilers as commercially made and sold the difference in quality of the plates and rivets, together with the great uncertainty as to the exact effect of punching the plates, have, so far, prevented anything like the determination either by calculation or experiment of what might be accepted as the best proportions of riveted joints.

In regard to steel plates for boilers Mr. F. W. Webb, of Crewe, England, Chief Engineer of the London and North-western Railway, has made over 10,000 tests of steel plates, but had only two plates fail in actual work; these failures he thought were attributable solely to the want of care on the part of the men who worked the plates up.

All their rivet holes for boilers were punched in a Jacquard machine, the plates then annealed, and afterwards bent in rolls; they only used the reamer slightly when they had three thicknesses of plate to deal with as in butt-joints with inside and outside covering strips. These works turn out two locomotive boilers every three days.

The Baldwin Locomotive Works, which turn out on an average three locomotives per day, punch all their rivet holes one-sixteenth inch less in diameter and ream them to driven rivet size when in place. They also use rivets with a fillet formed under head made in solid dies.

*Rivets.*—Rivets of steel or iron should be made in solid dies. Rivets made in open dies are liable to have a fin on the shank, which prevents a close fit into the holes of the plates. The use of solid dies in forming the rivet insures a round shank, and an accurate fit in a round hole. In addition,

there is secured by the use of solid dies, a strong, clean fillet under the head, the point where strength is most needed.

Commencing with a countersunk head as the strongest form of head, the greater the fillet permissible under the head of a rivet, or bolt, the greater the strength and the decrease in liability to fracture, as a fillet is the life of the rivet.

If rivets are made of iron, the material should be strong, tough and ductile, of a tensile strength not exceeding 54,000 pounds per square inch, and giving an elongation in *eight inches*, of not less than twenty-five per cent. The rivet iron should be as ductile as the best boiler plate when cold.



FIG. 15.



FIG. 16.

Iron rivets should be annealed and the iron in the bar should be sufficiently ductile to be bent cold to a right angle without fracture. When heated it should be capable of being flattened out to one-third its diameter without crack or flaw.

If rivets are made of steel they must be low in carbon, otherwise they will harden by chilling when the hot rivets are placed in the cold plates. Therefore, the steel must be particularly a low grade or mild steel. The material should show a tensile strength not greater than 54,000 pounds per square inch, and an elongation in *eight inches* of thirty per cent. The United States Government requirements are that steel rivets shall flatten out cold under the hammer to



the thickness of one-half their diameter without showing cracks or flaws; shall flatten out hot to one-third the diameter, and be capable of being bent cold in the form of a hook with parallel sides without cracks or flaws. These requirements were thought at first to be severe, but the makers of steel now find no practical difficulty in meeting these specifications.

The forming of the head of rivets, whether of steel or iron, and whether the heads are conical or semi-spherical, should not be changed by the process of riveting. The form of the head is intended to be permanent, and this permanent form can only be retained by the use of a "hold-fast," which conforms to the shape of the head. In the use of the flat hold-fast (in general use in a majority of boiler shops) the form of the head is changed, and if the rivet, by inadequate heating, requires severe hammering, there is danger that the head of the rivet may be "punched" off. By the use of a hold-fast made to the shape of the rivet head, this danger is avoided and the original form of the head is retained. This feature of the use of proper rivet tools in boiler shops has not received the attention it deserves. Practical use of the above-named hold-fast would soon convince the consumers of rivets of its value and efficiency.

The practice of driving rivets into a punched rivet hole from which the fin or cold drag, caused by the movement of the punch, has not been removed by reaming with a countersunk reamer, or better still a countersunk set, should be condemned, as by driving the hot rivet head down against the fin around the hole in the cold plate caused by the action of punching, the countersunk fillet is not only destroyed, but it is liable to be driven into the head of the rivet, partially cutting the head from the shank. If the rivet is driven into a hole that has been punched with a sharp punch and sharp die, the result is that the fillet is cut off under the head, and the riveted end is also cut, and does not give the clinch or hold desired. That is to say, rivet holes in plates to be riveted should have the burr or sharp edge taken off, either by countersinking, by reamer, or set.

*Heating of rivets.*—Iron rivets are generally heated in an ordinary blacksmith's or rivet fire having a forced blast; they are inserted with the points down into the fire, so that the heads are kept practically cool.

Steel rivets should be heated in the hearth of a reverberatory furnace so arranged that the flame shall play over the top of the rivets, and should be heated uniformly throughout the entire length of the rivet to a cherry-red. Particular attention must be given to the thickness of the fire in which they are heated.

Steel, of whatever kind, should never be heated in a thin fire, especially in one having a forced blast, such as an ordinary blacksmith's or iron rivet furnace fire. The reason for this is that more air passes through the fire than is needed for combustion, and in consequence there is a considerable quantity of free oxygen in the fire which will oxidize the steel, or in other words, burn it. If free oxygen is excluded steel cannot burn; if the temperature is high enough it can be melted and will run down through the fire, but burning is impossible in a thick fire with a moderate draft.

This is an important matter in using steel rivets and should not be overlooked; the same principle applies to the heating of steel plates for flanging.

*Riveting.*—There are four descriptions of riveting, namely:

- (1) Hammered or hand-riveting;
- (2) Snapped or set;
- (3) Countersunk;
- (4) Machine.

For good, sound work, machine riveting is the best.

Snapped riveting is next in quality to machine riveting.

Countersunk riveting is generally tighter than snapped, because countersinking the hole is really facing it; and the countersunk rivet is, in point of fact, made on a face joint. But countersinking the hole also weakens the plate, inasmuch as it takes away a portion of the metal, and should only be resorted to where necessary, such as around the front of furnaces, steam chests or an odd hole here and there to clear a flange, or something of that sort.

Hammered riveting is much more expensive than

machine or snapped riveting, and has a tendency to crystallize the iron in the rivets, causing brittleness.

In the present state of the arts all the best machine riveters do their work by pressure, and not by impact or blow.

The best machines are those of the hydraulic riveting system, which combines all of the advantages and avoids all the difficulties which have characterized previous machine systems; that is to say, the machine compresses without a blow, and with a uniform pressure at will; each rivet is driven with a single progressive movement, controlled at will. The pressure upon the rivet after it is driven is maintained, or the die is retracted at will.

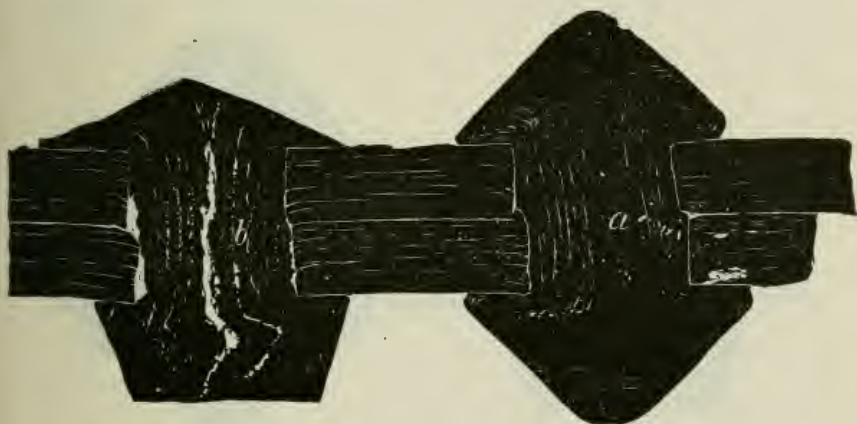


FIG. 17.

Hydraulic riveting has demonstrated not only that the work could be as well done without a blow, but that it could be *better done without a blow*, and that the riveted material was stronger when so secured than when subjected to the more severe treatment under impact.

What is manifestly required in perfect riveting is, that the metal of the rivet while hot and plastic shall be made to flow into all the irregularities of the rivet holes in the boiler sheets; that the surplus metal be formed into heads as large as need be, and that the pressure used to produce these results should not be in excess of what the metal forming the boiler shall be capable of resisting.

It is well known that metals, when subjected either cold or hot, to sufficient pressure, will obey almost exactly the same laws as fluids under similar conditions, and will flow into and fill all the crevices of the chamber or cavity in which they are contained. If, therefore, a hot rivet is inserted into the holes made in a boiler to receive it, and is then subjected to a sufficient pressure, it will fill every irregularity of the holes, and thus fulfil one of the conditions of perfect riveting. This result it is impossible to accomplish with perfection or certainty by ordinary hand riveting, in doing which the intermittent blows of an ordinary hammer are used to force the metal into the holes. With a hydraulic riveting machine, however, an absolutely uniform and con-

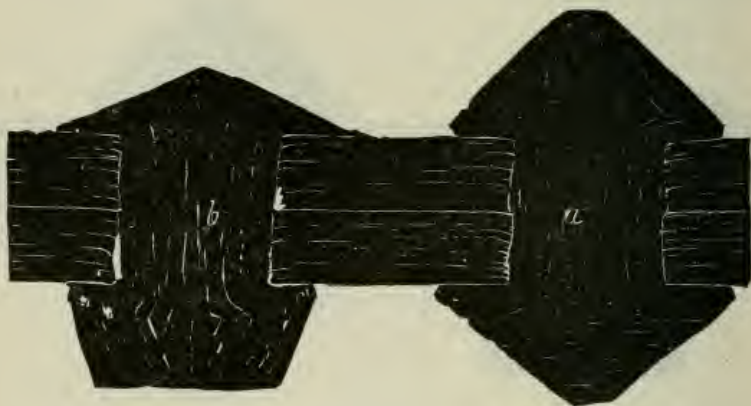


FIG. 18.

tinuous pressure can be imparted to each rivet, so as to force the hot metal of the rivet into all the irregularities of the holes in the same way as a hydraulic ram will cause water to fill any cavity, however irregular.

In order to illustrate the relative advantages of machine over hand riveting, two plates were riveted together, the holes of which were purposely made so as not to match perfectly. These plates were then planed through the centre of the rivets, so as to expose a section of both the plates and rivets. From this an impression was taken with printer's ink on paper and then transferred to a wooden block, from which *Figs. 17 and 18* were made.



The machine-driven rivet is marked *a*, and *b* represents the hammered rivet.

It will be observed that the machine rivet fills the hole completely, while the hand rivet is very imperfect. This experiment was tried several times, with similar results each time.

The hand rivet, it will be observed, filled up the hole very well immediately under the head formed by the hammer; but sufficient pressure could not be given to the metal—or at least it could not be transferred far enough—to affect the metal at some distance from the driven head. So great is this difficulty that in hand riveting much shorter rivets must be used, because it is impossible to work effectively so large a mass of metal with hammers as with a machine. The heads of the machine rivets are, therefore,

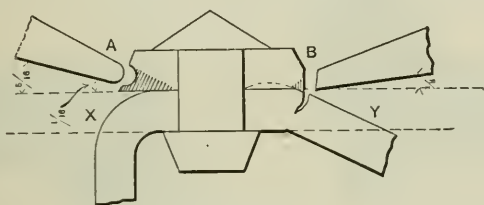


FIG. 19.

larger and stronger, and will hold the plates together more firmly than the smaller hammered heads.

To drive rivets by hand, two strikers and one helper are needed in the gang, besides the boy who heats and passes the rivets; to drive each five-eighths-inch rivet, an average of 250 blows of the hammer is needed, and the work is but imperfectly done. With a machine, two men handle the boiler, and one man works the machine; thus, with the same number of men as is required in riveting by hand, five rivets are driven each minute.

The superior quality of the work done by the machine would alone make its use advantageous; but to this is added greatly increased amount of work done.

The difference in favor of the riveting machine over hand riveting is at least *ten to one*.

In a large establishment a record of the number of rivets



driven by the hand-driving gang, also by the gang at the steam-riveting machine for a long period of time, in both cases making no allowances of any kind for delays. The rivets driven per month by each was—for the hand-driven rivets, at the rate of twelve rivets per hour, and for the machine-driven rivets, 120 per hour. In the case of the hand-driven rivets the boiler remains stationary and the men move about it, while the machine-driven rivets require the whole boiler to be hoisted and moved about at the riveting machine to bring each hole to the position required for the dies. Notwithstanding the trouble involved in handling and moving the boiler, it shows that it is possible

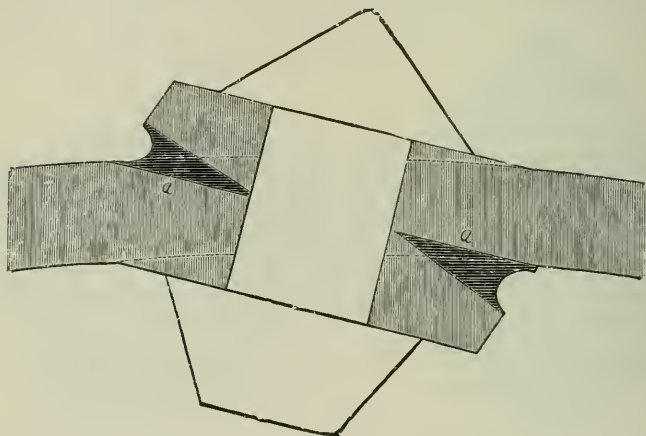


FIG. 20.

to do ten times as much work, and with less skilled labor, by the employment of the riveting machine.

*Calking.*—One great source of danger in boiler making is excessive joint calking—both inside and out—where a sharp-nosed tool is employed, and for the reason that it must be used so close to the inner edge of plate as to indent, and in many cases actually cut through the skin of the lower plate. This style of calking puts a positive strain upon the rivets, commencing distortion and putting excessive stress upon rivets—already in high tension before the boiler is put in actual use. It is, I hope, rapidly becoming a thing of the past.

With a proper proportion of diameter and pitch of rivet all that is required is the use of a light "fuller tool" or the round-nosed tool used in what is known to the trade as the "Connery system."

There is but little need of calking if means are taken to secure a clean metal-to-metal face at the joint surfaces. When the plates are put together in ordinary course of manufacture, a portion of the mill scale is left on, and this is reduced to powder or shaken loose in the course of riveting and left between the plates, thus offering a tempting opening for the steam to work through, and is really the cause of the heavy calking that puts so unnecessary a pressure on both plate and rivet. A clean metallic joint can be secured by passing over the two surfaces a sponge wet with a weak solution of sal-ammoniac and hot water, an operation certainly cheap enough both as to materials and labor required.

The above cut, *Fig. 19*, gives an illustration of calking done by sharp-nosed and round-nosed tools, respectively. It will be seen by *Fig. 20* that the effect of a round-nosed tool is to divide the plate calked, and as the part divided is well driven towards the rivets, a bearing is formed at *a*, from one-half to three-fourths of an inch, which increases the strength of joint, and will in no way cut or injure the surface of the under plate. A perfect joint is thus secured.

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## THE LAW OF VARIATION

OF THE THEORETICAL AMPLITUDE OF TIDAL OSCILLATION,  
AND THE ELEVATION OF THE HIGH WATER LINE IN THE  
VARIOUS CROSS-SECTIONS OF TIDAL RIVERS.

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BY L. D'AURIA.

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The problem of the variation of tidal oscillation or tidal range, from one cross-section to another of a tidal river has never been solved, and what is at present known on the subject is summed up in the vague notion that the tidal range generally increases when the width of cross-section diminishes.

In the *United States Coast Survey Report of 1854*, on pages 171, 172, can be found the statement that "the height of the tidal wave in a channel of the form of a wedge, and of equal depth increases nearly in the inverse ratio of the square root of width;" a law which, were it true, would produce an infinitely great tidal range at the head of any tidal river which would happen to terminate into a point, irrespectively of either the length or the depth of such a river. Of course, we know that this is not in the least the law of nature, and we do not need therefore take further notice of it. The reference has been made only to show how confused and meagre the literature of the subject is, without any intention to criticise.

In this investigation the following two hypotheses or propositions are involved :

(1) The tidal range in any given cross-section of a tidal river is supposed to remain unchanged were such cross-section completely occupied by a dam.

(2) The volume of water confined between any two given cross-sections of a tidal river at the instant when high water occurs, either in one or in the other of these cross-sections is the same.

These two propositions are, no doubt, admissible when the river is supposed to be of a uniform width, and there seems to be no reason why they should not hold good also for rivers of variable width.

Basing upon this assumption let  $MM$  and  $NN$  (Fig. 1), represent two cross-sections of a tidal river, and let  $MM_1 = R$  represent the tidal range of the lower cross-section  $MM$ ;  $NN_1 = \rho$  the theoretical tidal range of the upper cross-section  $NN$ ;  $M_1M_2 = y_1$  the elevation of tide in the lower cross-section at the instant when high water occurs in the upper cross-section;  $L$  the distance between the two cross-sections;  $A$  the area of the basin enclosed between them; and  $x_0$  the distance of the centre of gravity of such basin

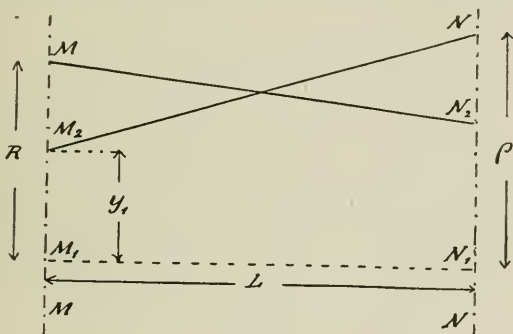


FIG. 1.

measured from the lower cross-section  $MM$ . Then the volume of water  $NM_2M_1N_1$  can be expressed by

$$q = Ay_1 + A \frac{x_0}{L} (\rho - y_1) \quad (1)$$

In accordance with the second proposition above enunciated this volume should be equal to the volume  $MN_2N_1M_1$ , and this, according to the first proposition, ought to be equal to the tidal volume that may be admitted up to the time of high water into a tidal river of length  $L$ , and area  $A$ , through the cross-section  $MM$ . This latter tidal volume can be computed by considering the tidal basin of uniform width  $b$  equal to the width of the lower cross-section, and of length  $s = A \div b$ . (See "Analytical Discussion of the Tidal

Volume," etc., by the author, *Journal Franklin Institute*, April, 1891). Let then  $y$  represent the ordinate of the tidal wave (*Fig. 2*), in this ideal tidal river at the distance  $z$  from the crest  $M$ , the height of the wave being  $R$ . Then, with the same degree of approximation as adopted in the previous computation the required tidal volume can be expressed by

$$q = \frac{1}{2} A (R + y) \quad (2)$$

The value of  $y$  is given by the equation

$$y = R \sin^2 \left( \frac{\pi x}{\lambda} \right)$$

in which  $x$  represents the distance from the trough of the

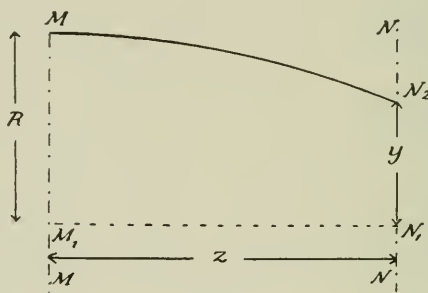


FIG. 2.

wave, or  $x = \frac{1}{2} \lambda - z$ . By substitution and reduction we obtain

$$y = R \cos^2 \left( \frac{2z}{\lambda} \right) \frac{\pi}{2}$$

Since  $z = A \div b$ , if we denote by  $b_0$  the average width of the basin we get

$$z = L \frac{b_0}{b}$$

and

$$y = R \cos^2 \left( \frac{2L b_0}{\lambda b} \right) \frac{\pi}{2}$$

Now, if we denote by  $\theta$  the time occupied by the wave crest to travel the distance  $L$ , and by  $T$  the mean interval



of rise and fall of tide, which is about 372 minutes, we can put

$$\frac{2L}{\lambda} = \frac{\theta}{T}$$

and

$$y = R \cos^2 \left( \frac{\theta b_0}{T b} \right) \frac{\pi}{2} \quad (3)$$

Substituting this value in equation (2) and then comparing with equation (1), we find

$$\rho = \frac{R L}{2 x_0} \left\{ 1 + \cos^2 \left( \frac{\theta b_0}{T b} \right) \frac{\pi}{2} \right\} - y_1 \left( \frac{L}{x_0} - 1 \right) \quad (4)$$

When the basin is rectangular, we have  $b_0 = b$ ,  $2x_0 = L$ , and

$$y_1 = y = R \cos^2 \left( \frac{\theta}{T} \right) \frac{\pi}{2}$$

Substituting these values in (4) we get  $\rho = R$ , a result which was to be expected.

If we denote by  $b_1$  the width of the upper cross-section, and express the width of the basin at any distance  $x$  from the lower cross-section by

$$r = b_1 + (b - b_1) \left( 1 - \frac{x}{L} \right)^n$$

then

$$x_0 = \frac{\int_0^L r x dx}{\int_0^L r dx}$$

and

$$b_0 = \frac{1}{L} \int_0^L r dx$$

Substituting the value of  $r$ , and integrating, we find

$$x_0 = L \frac{2b + n b_1 (n + 3)}{2(n + 2)(b + n b_1)} \quad (5)$$

and

$$b_0 = \frac{b + n b_1}{n + 1} \quad (6)$$

With these values equation (4) becomes

$$\rho = \frac{R (n + 2) (b + n b_1)}{2 b + n b_1 (n + 3)} \left[ 1 + \cos^2 \left\{ \frac{\theta}{T} \frac{b + n b_1}{(n + 1) b} \right\} \frac{\pi}{2} \right] - y_1 \frac{(1 + n) (n b_1 + 2b)}{2 b + n b_1 (n + 3)} \quad (7)$$

When the basin is trapezoidal in shape, then  $n = 1$ , and we have

$$\rho = \frac{3 R (b + b_1)}{2 (b + 2 b_1)} \left[ 1 + \cos^2 \left\{ \frac{\theta}{2 T} \left( 1 + \frac{b_1}{b} \right) \right\} \frac{\pi}{2} \right] - y_1 \frac{2 b + b_1}{2 b_1 + b} \quad (8)$$

and when the basin is triangular in shape,  $b_1 = 0$ , and

$$\rho = \frac{2}{3} R \left\{ 1 + \cos^2 \left( \frac{\theta}{2 T} \right) \frac{\pi}{2} \right\} - 2 y_1 \quad (9)$$

In all these formulas  $\theta$  is supposed not to exceed the value of  $T$ ; but suppose we have the case of  $\theta = T + \theta_1$  then we have to find first the value of  $\rho$  which corresponds to  $\theta - \theta_1 = T$ , and afterwards, with this value of  $\rho$  put instead of  $R$  and  $\theta_1$  put instead of  $\theta$ , our equations will furnish the value of  $\rho$  corresponding to  $\theta$ . In practice, however, in order to ascertain the variation of the theoretical tidal oscillation from cross-section to cross-section in a tidal river we have to divide such river in sections of such length that the curvature of the tidal wave in each section may be neglected.

When  $\theta = T$ , the value of  $y_1$  becomes zero, and the formulas (7), (8) and (9) become, respectively,

$$\rho = \frac{R (n + 2) (b + n b_1)}{2 b + n b_1 (n + 3)} \left( 1 + \cos^2 \left\{ \frac{b + n b_1}{(n + 1) b} \right\} \frac{\pi}{2} \right) \quad (10)$$

$$\rho = \frac{3 R (b + b_1)}{2 (b + 2 b_1)} \left( 1 + \cos^2 \left\{ \frac{1}{2} \left( 1 + \frac{b_1}{b} \right) \right\} \frac{\pi}{2} \right) \quad (11)$$

and

$$\rho = \frac{9}{4} R \quad (12)$$

If we put  $b_1 = 0$  in equation (10) we get

$$\rho = \frac{1}{2} R (n + 2) \left\{ 1 + \cos^2 \left( \frac{1}{n + 1} \right) \frac{\pi}{2} \right\} \quad (13)$$

which clearly shows that  $\rho$  increases with  $n$ . Hence, in a trumpet-shaped tidal river or estuary, the range of tide increases more rapidly than if such river or estuary were triangular.

When the value of  $b$  in equation (4) cannot be accurately determined, owing to sudden and irregular variations of width, then it will be better to substitute in such equation the value of  $\frac{b_0}{b}$  expressed in function of  $x_0$  and  $L$  as given by the formulæ (5) and (6), viz:

$$\frac{b_0}{b} = \frac{L}{(n + 3) L - 2 (n + 2) x_0} \quad (14)$$

and we get

$$\rho = \frac{R L}{2 x_0} \left\{ 1 + \cos^2 \left( \frac{\theta}{T} \frac{L}{(n + 3) L - 2 (n + 2) x_0} \right) \frac{\pi}{2} \right\} - y_1 \left( \frac{L}{x_0} - 1 \right) \quad (15)$$

If the portion of river under consideration be rather short we can put  $n = 1$ , and then we have

$$\rho = \frac{R L}{2 x_0} \left\{ 1 + \cos^2 \left( \frac{\theta}{2 T} \frac{L}{L - 3 x_0} \right) \frac{\pi}{2} \right\} - y_1 \left( \frac{L}{x_0} - 1 \right) \quad (16)$$

When, however, the variation of width in the length  $L$  is such as to show a decided curvature, then we can, by few tentatives, find out which value of  $n$  better corresponds to it; but in practice it will be found that where the curve presents its convexity to the stream, for a short portion of river, we can put  $n = 2$ ; and when the curve presents its concavity to the stream, we can put  $n = \frac{1}{2}$  in equation (15) to obtain the corresponding value of  $\rho$ .

The tidal oscillation  $\rho$ , as given by the above formulæ, would obtain in rivers perfectly free of resistances, and would take place one-half above and one-half below a common level plane for the whole length of the river. As it is, however, there is always a greater or less volume of water held back during the ebb discharge, on account of the various resistances of the river bed, and this volume of water, without affecting the position of the high-water line, has the effect of raising the low-water line above its theoretical position. Now, it is easy to see that the difference of elevation of high-water between any two given cross-sections of a tidal river can be computed by the formula

$$\delta = \frac{1}{2} (\rho - R) \quad (17)$$

which offers a simple and accurate method of running a level line along any tidal river entirely by means of tidal observations.

The writer had in mind to test the accuracy of this method by comparison with some well-established level line along a tidal river, but a sudden change in his professional occupation having made it difficult for him to get at the necessary data this part of his work had to be postponed for some future time. The following example, however, though only computed with roughly estimated data, may serve to give some idea of the plausibility of the results which may be expected from the above method.

In 1870, the United States Coast Survey, by means of levels of precision, determined the elevation of mean level (half tide) at League Island Navy Yard, on the Delaware River, above the mean level (half tide) at Keyport, N. J., and it was found to be 3.3 feet. The mean level at the entrance of Delaware Bay can be considered practically the same as that at Keyport; and since the range of tide at League Island is given at 6.25 feet, and at the Delaware Bay as nearly 5.0 feet, we would have for the difference of elevation of high water between League Island and Delaware Bay,

$$\delta = 3.3 + \frac{6.25}{2} - \frac{5}{2} = 3.92 \text{ feet.}$$

From the entrance of Delaware Bay up to League Island the value of  $\theta$  is nearly equal to  $T$ , so that we can put  $y_1 = 0$  in equation (4) which then gives

$$\rho = \frac{R L}{2 x_0} \left\{ 1 + \cos^2 \left( \frac{b_0}{b} \right) \frac{\pi}{2} \right\}$$

The values of

$$\frac{L}{x_0} \text{ and } \frac{b_0}{b}$$

have been approximately estimated at four and two-thirds, respectively, which offer

$$\rho = 2.5 R$$

Substituting in equation (17) we get

$$\delta = 0.75 R$$

and since  $R = 5$  feet, we have

$$\delta = 3.75 \text{ feet.}$$

This differs from the value deduced from the Coast Survey levels of precision for less than two-tenths of a foot.

Denoting by  $\rho_1$  the actual observed value of  $\rho$  we can form some idea of the efficiency of the river bed so far as the freedom of flow is concerned, by consulting the following ratio or coefficient of efficiency, viz:

$$\epsilon = \frac{\rho - \rho_1}{\rho} \quad (18)$$

In the case of the Delaware River just considered, the theoretical range of tide would be  $\rho = 12.5$  feet. Hence, with  $\rho_1 = 6.25$  feet we get  $\epsilon = 0.5$ .

The removal of obstructions and the deepening of the channel of a tidal river have the tendency of bringing  $\epsilon$  nearer to *one*: but in no case such limit can be practically reached. When such improvements are effected on a tidal river, the value of  $\rho_1$  is increased by the lowering of the actual low-water line toward its theoretical position without affecting the position of the high-water line. This conclusion is found to be corroborated by a wide experience in the improvement of tidal rivers.



## THE LIMITS OF SCIENTIFIC INQUIRY.

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## I. GENERALITIES AND DEFINITIONS.

It is a peculiarity of natural science that its most surprising revelations are to be found in quarters where they are often least suspected, and even the most dissimilar departments of inquiry are so interlinked, so closely connected by invisible threads, as it were, that it is almost a matter of indifference what object we take in hand, whether a butterfly, or the first pebble we come across; a piece of chalk, or a piece of charcoal; a meteorite, or a star-fish, we shall find a sermon in it. We shall find—if we carry our inquiry far enough—that the butterfly and the pebble are not so very different in kind after all; that there is more than a superficial connection between a piece of chalk and one of charcoal, and that even such dissimilar things as meteorites and star-fishes show points of analogy where we may have least expected them.

We are living in a world of mystery. We are surrounded by appearances which we fondly persuade ourselves to be realities, having from infancy been accustomed to look upon them as such, though they are little more than phantoms of our imagination, or, as Carlyle aptly expressed it, the clothes in which we dress our ideas. On the other hand, there are a number of phenomena which, by reason of their very grandeur, should inspire us with awe, and give us food for unceasing reflection. We see the sun rise in the east and set in the west, and look upon this as the most natural thing in the world; we see the moon with its changes, and all the starry host—shooting-stars, and now and then a comet, and habit has rendered us indifferent to these phenomena. We drop a stone and expect it to fall upon the ground, we plant an acorn and expect an oak-tree from it: we would be greatly surprised, indeed, to see it develop into

a cucumber; we perceive the effects of heat and cold upon ourselves and other bodies, and look upon all these things as matters of course—yet they are profound and awful mysteries if we look below the surface.

Carlyle, in his *Sartor Resartus*, gives utterance to the following remarkable sentence: "I stretch forth my hand to clutch the sun. Vain and foolish attempt! It is 100,000,000 miles distant. Yet, if by some wondrous miracle, my arm were to extend, of a sudden, so that I could touch it, the marvel would merely consist in the sudden lengthening of my arm, and in the power of my body to sustain it: it would be incomparably smaller than the marvel that I am here and have an arm which I can stretch forth at all."

We are surrounded by mysteries. Our existence itself is a mystery, and in proportion as we recognize this fact, we become restless and dissatisfied. We experience an indescribable longing to solve these mighty problems, to grasp the why and how of things, to fathom the infinite—a yearning and striving which not seldom becomes a burning desire, and develops into an all-absorbing passion. This feeling exists, it is real, and is implanted, I venture to say, in every human heart, although the majority may be unconscious of it. It manifests itself not only in the speculations of the philosopher, but in the general thirst for knowledge—in the curiosity of women as well as in the inquisitiveness of children. This universal longing for the unknown, this insatiable desire to ascertain facts and draw conclusions: what is it but our innate tendency to solve the riddle of existence, which seems to be the common inheritance of mankind?

The riddle of existence has not been, and probably never will be, completely solved, although it would be an unwarrantable dogmatical assertion to pronounce as absolutely impossible so gigantic a triumph of the human mind. But it may comfort us to know that we are at last marching in the right direction. We are no longer groping blindly in the dark, and, though we may never reach the goal, yet we can approach it. We can approach that glorious goal,

and we are approaching it. Our progress during the last 100 years is not merely visible, it is vast. It is marked by a succession of the most wonderful discoveries, of marvellous truths, which have flashed upon us; which have widened our horizon beyond the wildest dreams of eighteenth-century philosophers, and there is every indication that this progress will continue in a geometrical ratio.

## 2. THE OLD AND NEW PHILOSOPHY.

Our methods of inquiry differ vastly, if not totally from those of old, from those pursued by the old-world philosophers, with few exceptions, from Plato to Descartes. We take nothing for granted. We do not raise our fabric on a dogmatical assumption however seductively it may allure us. We draw our conclusions from experience, from facts which may be tested and verified at any moment: we require no belief, and we scorn to appeal to unreasoning "faith" of any description.

The difference between the old and new method is this: The inquirers and philosophers of the past imagined that they could arrive at the truth by a mere process of reasoning, independent of the phenomena of nature which surrounded them. They based their ingenious structures on the monstrous assumption that thought or mind existed altogether distinct from matter, that mind and matter were two separate existences, which have nothing whatever in common. They believed that by mere mental efforts they could solve the secrets of the universe, independent of matter, its laws and properties. Hence a science in our sense was, with them, impossible. They treated the phenomena of nature with indifference, nay with undisguised contempt, as unworthy of their notice, they relied solely on the "mind" to account for all that is hidden. Thus all the systems which they compiled, based on this rotten foundation, however ingenious in themselves, are monstrous absurdities, quite astounding and amusing if examined in the light of modern science. They are interesting only in so far as they afford an appalling example of misdirected and lost energy, and by warning us to avoid the rocks on which they grounded.

We have abandoned this hopeless dominion; we have struck out a road for ourselves. We do not believe that mind and matter are separate existences—in fact, we do not “believe” anything. To us the word “belief” is devoid of meaning; we either know or are in a state of agnosis. We have strong grounds for assuming that what is called “mind” is only a property of matter, just as elasticity, expansibility and gravity are properties of matter, and it is to matter that we are looking for redemption. We do not analyze and dissect the mind, we analyze and dissect matter. The modern philosopher studies physiology before he ventures an opinion on psychology: witness Herbert Spencer.

We observe the laws and phenomena of nature, we investigate the character of the objects which surround us and draw from them our conclusions. We know and recognize no other guide. If our deductions are hostile to theology the hostility is not intentional on our part, inasmuch as it is not, from the outset, our motive to destroy religious beliefs. If our labors should tend to do so, we can only shrug our shoulders. We want to proclaim the truth, quite irrespective of the consequences; we have nothing whatever to do with this subject in the prosecution of our task, we leave it entirely alone. To say, for instance, there is no creator, is in our eyes quite as dogmatical as to say there is one, but we decline to resort to the supposition of a deity in order to account for those mysteries which have, as yet, eluded our grasp. If, in order to account for the origin of a world, the disciple of religion points to a deity we only remind him that by doing so he explains nothing. He only substitutes a greater mystery for one already appalling in its magnitude. He reminds us of the Hindoo fable in reference to the “foundations of the universe,” expressed in stone on every Hindoo temple. We there behold the figure of an elephant, carrying on its back a huge disc, which represents the world. The elephant, again, is supported by a still more gigantic tortoise. The speculative Hindoo is not so much interested in the question of the world’s creation as in the problem of its foundation or support.



"What does the world rest on?" we ask a Brahmin.

"An elephant."

"And the elephant?"

"On a tortoise."

"And the tortoise?"

"That supports itself; it rests on nothing."

Supports itself? rests on nothing? Would it not have been much simpler to make the world self-supporting at once? Why introduce the elephant and the tortoise? They explain nothing; on the contrary, they only increase the world's weight and our difficulty.

Modern science discards every species of dogma and ignores every kind of supernatural intervention. Modern science is not conservative and pedantic, it is progressive and courts publicity; it is open to all and offers the purest and noblest pleasures, while it elevates the mind and makes veritable kings of its disciples.

### 3. THE GROWTH OF SCIENTIFIC KNOWLEDGE.

Geology is one of the most charming of the natural sciences, but, at the same time, it is one of the most difficult, because it involves questions and problems of the most varied and complicated character. Extensive preliminary studies in the departments of mineralogy, chemistry, physics and even astronomy are necessary before a broad general conception of this great science can be acquired by the student. There was a time—less than fifty years ago—when even great and wealthy colleges employed only one man to teach the whole of the natural sciences. To be sure they always had their full complement of historians, theologians, professors of literature, and, above all things, a complete set of philologists to take care of those very tremendous subjects, Greek and Latin. They were very expensive, these "classical" languages, but then they possessed the advantage of being comparatively useless which in itself was sufficient to insure them the highest esteem of the average university-regent.

The natural sciences were relegated to one man, generally poorly paid, who was styled "Professor of Natural Science,"



and who was expected to teach, and know all about, physics, chemistry, astronomy, geology, biology; in short, everything which comes within the scope of natural science. At the present day each of these subjects, and a dozen others, forms a distinct and separate study, and the labors of an army of indefatigable workers in each have already resulted in the accumulation of an amount of scientific literature, the mastering of which even in its most essential features, taxes the endurance of the most persevering.

To-day we have no professors of natural science at our large colleges, but we have professors of astronomy, of geology, of mineralogy, of chemistry, physics, etc., for each of these subdivisions has grown into a great science for itself, which requires the study of a life-time. The science of geology alone has assumed such vast proportions that no ordinary intellect can master it in all its bearings. Thus we now have palæontologists, men who devote themselves entirely to the study of fossils, mineralogists who inquire into the forms and properties of minerals, and petrographers who examine into the structure of rock-masses, and each of these specialties is capable of still further subdivision, and where it will end is more than I dare conjecture. If we go to any of our great colleges we shall find that the professor of botany, for instance, knows comparatively little beyond his own specialty; his knowledge of the other departments of natural science is sometimes not much better than that of the average school-boy's. It is sad that it should be so, but it is the inevitable consequence of the growth of knowledge. We can no more expect a professor of botany to know all about geology, chemistry, or natural philosophy, than we can expect a pint measure to hold a bushel. These men are the intellectual hewers of wood and drawers of water, the compilers of raw material, and it happens only once or twice in a century that a master-mind appears, like Laplace or Isaac Newton, like Darwin or Herbert Spencer, who surveys the whole gigantic field, like some general, who connects the accumulated facts and links them, and traces a wonderful law, of which no one had dreamed, which is listened to by mankind with bated breath, and received as a new and glorious revelation.

## 4. GEOLOGICAL PARADOXES.

We will now endeavor to present or discuss one or two problems in reference to the structure of our earth, or better, perhaps, in reference to the condition of the earth's interior, which may be new to many, and which, if they are not altogether new to those who have studied geology in its broader aspects, in connection with astronomy, physics and chemistry, will at least interest them in so far as the writer may succeed in giving an explanation of certain phenomena which have not as yet, to his knowledge, been accounted for in popular language. Besides, it may be presumed that an object looks different according to the direction in which we view it, and the greater the number of these directions, and the more varied the points of view the more perfect and accurate will be our impression of the size, shape and character of that object.

This globe, our earth, which we inhabit, and which revolves once in twenty-four hours around its axis, while pursuing at the same time its great course around the sun, and onward with the entire solar system towards an unknown region of space—probably around some still greater centre of attraction—this earth of ours has a diameter of about 8,000 miles. We live on its surface, on the surface of its outer crust, as it has been termed. We are tolerably well acquainted with this surface; we have travelled nearly all over it and have explored it in every direction, even though the highest polar regions and some parts of Central Africa are still unknown to us. We know that about three parts consist of water, in the shape of oceans and seas, and one part of what we are accustomed to call land. We find on this land mountains and valleys, and strange configurations, we find it rising from the ocean level, higher and higher, until it reaches in the loftiest summits with which we are acquainted an elevation of nearly 30,000 feet. We find this land, on the other hand, depressed in certain localities, depressed hundreds of feet below the sea-level (the dry land, of course; the coast of the Black Sea is about 200 feet beneath the level of the Mediterranean and the shore of the Dead Sea, 1,000 feet lower still). We have bored and dug

into it to a depth of some thousands of feet, yet our acquaintance with this terrestrial globe is but an exceedingly slight one; it only extends to a most insignificant portion of its outer crust. What are the highest mountains and what are the deepest borings, compared with the enormous bulk of our planet; what is 30,000 feet; what is 100,000 feet, compared with the radius of 4,000 miles? The loftiest Himalaya peaks, if contrasted with the diameter of the earth are but as the little protuberances on the skin of an orange, or the tiny granules on an egg-shell, and if we were to represent the Himalaya range on a globe a foot in diameter a few grains of rice, placed side by side, would furnish more than all the height required.

What is the character of that unknown interior? What kind of materials are hidden in these immense regions below and concealed from our sight forever? This is one of the fundamental problems of geology, a problem which has occupied the minds, not only of geologists but of physicians and astronomers for more than a century. We cannot get at those substances of our earth's interior which are more than a mile or two removed from the surface, and shall probably never be able to ascertain their condition by direct observation; yet we know that these substances must be quite different from those which we find at the surface, viz: in the insignificant portion of the outer crust, which we have been able to explore. It has been found that the average density of the earth is a little over five and a half, in other words the earth weighs about five and a half times as much as a globe of the same size, but consisting of water, would weigh. Now, it is certainly curious that the average gravity of the crust—so far as we have been able to explore it—is only two and a half; all the different kinds of sedimentary, metamorphic and eruptive rocks, minerals, fossils, etc., in short, all the materials of which the outer crust is composed weigh, on an average, only two and a half times as much as water. We are thus driven to the conclusion that the interior of the earth is composed of substances which have more than twice the density of those which constitute the outer crust. So far, we are not dealing

with anything in the nature of a theory or mere speculation, but with facts which can be as clearly proved as that twice two are four.

Now, in order to account for this enormous weight or high specific gravity of the earth, it has been assumed—and there is not now, perhaps, a single progressive geologist who doubts it—that during the gradual cooling or condensation of our globe the heavier substances gravitated towards the centre, and that the others were deposited around these according to their various densities. We are now acquainted with about seventy different fundamental substances or “elements,” among which iridium is possessed of the highest specific gravity (weighs twenty-two times more than its bulk of water, thus a little more even than platinum). Now, provided there are not elements still heavier in the vastnesses below, which is extremely probable, we may consider the structure of our planet as that of a series of concentric shells, with a nucleus of iridium. This nucleus may have a diameter of from fifty to 200 miles, it is surrounded by a layer of platinum, followed by gold, lead, silver, copper, iron, etc.—the various metals according to their specific gravity—and finally the lighter elements, such as aluminium, silicon, magnesium and calcium (the latter one of the lightest, its gravity being only one and a half). We may, indeed, safely conclude that this succession of concentric layers, or shell-structure, is the structure of this terrestrial globe, viz: that the various materials are arranged or deposited according to their densities, and it would seem as if the greater part of the earth’s interior—the thickest of all the shells—consisted of metallic iron. (It will be readily understood why most, if not all the metallic elements, which constitute the inner shells, must exist in a pure metallic state, if we bear in mind that oxygen, as one of the lightest elements, can only exist on or near the surface, and can only there undergo its combinations.)

It was formerly believed that the earth’s interior was in a molten condition, that our planet was, so to speak, a drop of fiery liquid, surrounded by a comparatively thin crust, and the numerous volcanoes, which we find in or on this



crust, were looked upon as the chimneys of the huge inferno, or as safety-valves for letting out steam and other superfluous products. This absurd theory—absurd in the light of modern science—has been abandoned by every philosophical inquirer of the present, and is only retained by a few geologists of the old school, who have written bulky volumes thirty or forty years ago, full of dogmatical assertions, from which—for reasons best known to themselves—they cannot now well depart: men who, during the last thirty years, have neither learned nor forgotten anything, who have not progressed a single inch and still expect to live on the fame which they acquired at the time of our grandfathers, when it was far easier to establish a reputation in science than now.

We can now show, by an overwhelming array of incontrovertible arguments, that the earth must be solid to the very core. We are driven to infer this from the enormous gravity of our planet, from its small diameter, from the necessary increase of pressure towards the centre and from numerous other data. Besides, it has been experimentally demonstrated that when a globular molten mass is allowed to cool the solidification invariably commences at the centre and advances toward the periphery, a fact which had been theoretically established by mathematicians and physicist long before the experiments were undertaken. The lavas and other eruptive products ejected by volcanoes have nothing whatever to do with the earth's interior—with that greater interior which here concerns us—they are derived from an insignificant depth in the crust and are only local phenomena, which will be explained later on.

But here we are confronted by a most remarkable paradox. If we dig down, or bore into, the earth's crust no matter where—we observe an increase of temperature which averages  $1^{\circ}$  (Centigrade) for every ninety feet. Thus if we go down 180 feet we have  $2^{\circ}$  more than at the surface, if we go three times ninety,  $3^{\circ}$  more, etc. The regularity and constancy of this increase of temperature in proportion to the depth is quite astonishing and down to the greatest depth which has, as yet, been reached by boring or mining this law has been



found to prevail. In all the deeper mines, as, for instance, many coal mines in England, Wales and on the continent of Europe, as well as some of the silver mines of Nevada and California the heat is so unbearable that the workers have to be relieved after short intervals.

Now, if this increase of temperature continues at the same rate towards the earth's centre, which cannot well be doubted, then at a depth of less than five miles the heat must already be beyond that of the boiling-point of water, at thirty miles depth every known rock must exist in a state of fusion, at fifty miles even platinum, a metal which we can only melt with the greatest difficulty; at a depth of 200 miles we would have such a fabulous temperature that every known substance could only exist in a most attenuated state, as a gas, and what the condition of matter may be at 2,000 miles depth is more than we dare even hint at; it may be presumed, however, that the explorer would run no risk of catching cold there and that he would get along well without an overcoat.

But how can we reconcile this with the limited diameter and the enormous gravity of our planet? With this gradual increase of temperature the materials, instead of becoming denser and heavier, should become lighter, more and more attenuated or gaseous, in proportion as we approach the central region. This would necessitate a far greater diameter of our globe, a diameter which should extend beyond the moon's orbit, a diameter of at least 500,000, instead of 8,000 miles.

This very remarkable apparent incongruity has been a great puzzle to geologists and physicists, and it was only a few years ago that the mystery was solved in so thorough and simple a manner that many have not yet quite recovered from their astonishment, or rather from mortification in contemplating their previous blindness.

We know that the melting-point, boiling-point or temperature under which any given substance will assume the gaseous state, is determined by the pressure exerted upon it, either by the atmosphere or by any other body. Water, for instance, boils at 100° (Centigrade), but if we ascend

a mountain and heat it there, it will boil earlier, viz: it will require less than  $100^{\circ}$  of heat to make it boil, and the higher we go the more quickly ebullition will ensue. What is the cause of this?

The atmospheric pressure is less. All the air beneath us—perhaps a stratum a mile or more in thickness—no longer presses on the water, which on that account, can expand and evaporate more readily. On the summit of Mt. Blanc, for instance, water will boil already at  $85^{\circ}$  (instead of  $100^{\circ}$ ). If we wanted to boil an egg there, we might keep on boiling it for hours, days, years or centuries and it would never be done, because at  $85^{\circ}$ , the albumen does not coagulate. In proportion as the atmospheric pressure diminishes the boiling-point of water is lowered, and here we have a ready means for determining the heights of mountains. All that is required is a test-tube, a spirit-lamp and a thermometer. A little water is boiled, then, by dipping the thermometer and noting how high the quick-silver column rises, the observer knows at once the approximate altitude. On the Plateau of Peru—perhaps the highest habitable region of our globe—no kind of food can be prepared by means of boiling. We could not cook an egg there, to say nothing of such things as potatoes, rice, peas, etc., neither soups nor stews are possible, the meat would remain almost raw in the water, and if we were to boil it for weeks in succession. Now if, on the other hand, we were to heat water below the level of the ocean, say in a deep mine, it would, of course, require a greater temperature than  $100^{\circ}$  to cause it to boil, and if we were to enclose it in a cylinder and exert upon it an artificial pressure of several atmospheres, we might expose it to a temperature which would suffice to melt iron, without reaching the boiling-point.

It will now readily be understood why the huge mass of our planet, in spite of the increasing temperature as we approach the centre, must be firm and solid throughout. The heat, of course, increases  $1^{\circ}$  per ninety feet of depth, but the pressure of each ninety-feet stratum increases in still greater proportion and more than counteracts the

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tendency to melt, boil or evaporate, on the part of the materials involved.

Now, if it should happen in the neighborhood of the surface, for lower down it is quite inconceivable that the pressure exerted upon the rock-masses (and which alone prevents them from melting, boiling or flying into space) is temporarily relieved, lightened or removed, in consequence of local disturbances, such as shiftings of strata, denudations, etc., then a certain region which, perhaps, occupied several cubic miles, may be almost instantaneously transformed into a volcanic reservoir. The previously solid mass will be changed into liquid lava which, of course, requires more room and forces its way upwards, causing earthquakes, volcanic outbursts and similar phenomena. The extinct volcanoes along the Rhine, in the Auvergne and at innumerable other regions of the earth's crust, indicate the former existence of such subterranean reservoirs, caused by a release of pressure, in consequence of local disturbances and volcanic regions of this kind may originate at any time and at any place. Such a reservoir requires centuries, or thousands of years, to cool again, as lava is a bad conductor of heat, but ultimately solidification must ensue.

If the vast bulk of our earth's interior were in a molten condition we might naturally expect that the lavas ejected by the numerous volcanoes would exhibit a general uniformity in structure and composition. Instead of this they present the greatest differences, which can only be accounted for by attributing to them a local origin. Thus the lava ejected by Mt. Chimborazo, in the Cordilleras, is a dense black substance, resembling basalt, while Mt. Cotopaxi, which is only a few miles distant, throws out a light porous pumice-stone. Moreover, the specific gravity of the lavas never exceeds three, thus they can only represent regions of the uppermost crust of our planet.

##### 5. IMMORTALITY IN THE LIGHT OF MODERN DYNAMICS.

The hypothesis in reference to the inevitable re-grouping of atoms, in accordance with the calculus of permutations, which I announced in conclusion of my lecture on

"Geological and Cosmical Problems," before the Franklin Institute, on November 17, 1890, is not entirely new, and I am bound to say that in at least one of its aspects it was advanced more than a hundred years ago by the great German philosopher, Leibnitz, at a time when the sciences of chemistry and physics were not sufficiently advanced to warrant such a speculation. In the light of modern dynamics, however, it deserves our closest attention, for if it can be shown that matter is composed of ultimate particles, call them atoms, centres of forces, or what we like, which are indestructible and in a state of continual vibration, I do not see how we can escape the conclusions which are forced upon us by this hypothesis. Some of the points which I am now about to discuss are new and I am not aware that this entire subject has ever been presented in the manner in which I now propose to deal with it.

According to the nebular hypothesis our earth, like all the rest of the planets, once existed in the shape of a gas-ring, which was thrown off, or became detached from, the sun during its process of condensation. This ring could not retain its form, it necessarily went to pieces, and these afterwards collected into a single gas-globe, or spherical mass, which kept on pursuing its course around the great central body. The gaseous globe radiated an enormous amount of heat, it grew denser and denser, while its diameter diminished, it underwent an endless series of metamorphoses, until it finally became the earth as we know it, the planet which has given us birth. So far all this is nothing new.

Now, even if the nebular hypothesis should prove erroneous the conclusions which I am now about to present will remain in force, for the same ultimate conclusions can be drawn from every other world-hypothesis which has, as yet, been advanced.

Every particle of our earth, every object, every substance which we now have upon or in our earth, must have already existed in that gaseous ring or primitive gas-globe, no matter in what form or condition; it was there. In that gas-globe were the particles which, after countless ages, became



united and roamed the great Mississippi valley in the shape of a mastodon, in that globe of gas were the atoms of carbon, which now constitute the table on which I am writing these lines, in that immense rotating sphere were the substances which are now united in the body of my humble self.

Could we but follow, in a few days or hours, the changes, the transformations, the endless pilgrimages, which the atoms and molecules of the substances had to undergo during those æons, before they became united so as to form, for instance, a human body, what marvels would we behold? The particles of hydrogen, carbon, phosphorus, etc., of which my body is composed, what a history might they not tell? In how many other bodies of the human species, of animals, plants and inorganic compounds may they not already have existed, separated, united, differently grouped or arranged? What may they not already have gone through and experienced? The reader will doubtless remember that famous church-yard scene in "*Hamlet*," in the fifth act, where *Hamlet* picks up a mouldering skull—the skull of poor *Yorick*—and addresses his friend *Horatio* thus :

*Hamlet* : To what base uses may we not return; why may not imagination trace the noble dust of Alexander till he finds it stopping a bung-hole?

*Horatio* : 'Twere to consider too curiously to consider so.

*Hamlet* : No, faith, not a jot, but to follow him thither with modesty enough and likelihood to lead it; as thus: Alexander died, Alexander was buried; Alexander returneth to dust; the dust is earth; of earth we make loam, and why of that loam whereto he was converted might they not stop a beer-barrel?

"Imperious Cæsar, dead, and turned to clay,

Might stop a hole to keep the wind away :

O, that that earth, which kept the world in awe,

Should patch a wall to expel the winter's flaw !"

If King Solomon, wise king though he was, really pronounced, or was the first to pronounce, the opinion that there is nothing new under the sun, he could not possibly have been aware of the enormous significance which attaches to this idea in the light of modern science. Why should not



the dust of Cæsar, which is now filling a bung-hole, why should not those atoms and molecules which, 2,000 years ago, were united in the body of Cæsar, why should they not, after endless transformations, endless changes, endless transitions, become again united in precisely the same manner; in other words, why should not the same Cæsar of whom we read in ancient history, reappear at a given time: in short, why should not everything now existing be compelled to undergo the same cycle of changes and reappear, not once, but an infinite number of times? It would be very strange if such were not the case. The following will illustrate this:

Supposing we were to take six dice, such as are used in the ordinary game. Let us place them in a little box, shake them and throw them on the table. We will assume that they had fallen so that each cube exhibited the number three on its upper face; of course, a rare chance. Now it can be mathematically shown after how many throws those six numbers are likely to reappear according to the law of chance. It is possible that they may turn up already with the next throw; on the other hand, we may have to cast those dice 10,000 times. Both cases are improbable; the probability lies in a certain number. If, instead of six dice, we were to take seven, the critical number is, of course, so much further removed, viz: it would be necessary to throw oftener to get the seven three's, and so the number of casts increases with every additional cube, till we finally obtain enormous figures. But no matter how many dice, the three's must turn up, if we can throw them long enough, and if, in the case of 1,000 dice, it were to take 1,000,000 years, the three's must appear and reappear again and again after proportionate intervals.

Supposing now that, instead of dice, we were to take a glass filled with sand. There are, let us assume, 20,000 sand grains in the glass. Each particular grain occupies a certain position, which is bound to differ from that of all the rest of the sand grains; this the reader will doubtless admit. We shake the glass; the positions are altered, the order of arrangement is disturbed. We shake it again; the sand

grains are now in a totally different position. We continue shaking the glass and the time must come when each individual grain again occupies the exact position which it occupied when we originally started. It is a mathematical necessity, which all will admit who know anything of the calculus of permutations. The 20,000 sand grains may be looked upon as so many dice, which are bound to fall precisely as they once fell if we can throw them sufficiently often.

Now, I have strong grounds for assuming that my body is composed of atoms, or groups of atoms, of a limited number of elementary substances, or of one elementary substance, if all matter has been evolved from one primary element. The number of these atoms may be ever so great, it has nothing whatever to do with the inevitable result. I know also that all other bodies are composed of such atoms, or groups of atoms (molecules), not only those of the human species, animals and plants, but of inorganic substances, rocks, metals, fluids, gases; in short, of everything which exists in, upon or above the ground in the atmosphere. I know, furthermore, that the atoms of even the hardest and seemingly most enduring substances, such as agate and diamond, are in a state of continual vibration, that nothing can permanently retain its form, that the entire universe always has been, is now, and always will be, in a state of metamorphosis or continual change.

The time must arrive when the atoms or molecules which are now united in my body, after countless transformations and wanderings through all kinds of bodies, substances or intermediary stages, will once more unite in the same manner; in other words, the time must arrive when my life, like that of every other individual, will repeat itself. Yes, repeat itself, and not merely once, but an infinite number of times.

And more than this. If one of my readers should imagine that the atoms or molecules, which now constitute his body, are thus associated for the first time, I can only admire his simplicity. There is nothing new under the sun. Those molecules were united in this manner before, and

before this again, and 100,000,000 times previously, as far as our imagination can carry us back into the abysmal night of the æons of the past. In other words: each of my readers has been, ages ago, what he is now, has lived and gone through all this before, has felt and experienced what he now feels and experiences, down to the minutest details, has opened his *Journal of the Franklin Institute* billions of years ago and read the same lines; not once, but an endless number of times. The recollection, of course, is lost. Life and mind itself, consciousness or "soul" is only a product of matter, and if the same substances reunite in the same manner, the same phenomena must inevitably recur.

Let the molecules which now constitute my body undergo ever so many metamorphoses, let them even—which, of course, is very improbable—once fill a bung-hole, let them be scattered about in all manner of forms and conditions, in close contact or millions of miles apart; they must come together again, may the thought please or distress me; this is the iron logic of modern dynamics.

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## PHENOMENAL FRICTION.

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BY JOHN H. COOPER.

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When making experiments during the month of February, 1891, with the Thurston railroad testing machine, I noticed the ease with which the axle-box could be made to slide longitudinally upon the axle when the same was in motion.

The several boxes tried had about fourteen square inches of surface in contact with the axle; they were variously loaded, with weight from 262 pounds upwards, and the axle was running at speeds varying from 160 to 400 revolutions per minute.

One box could be moved by a pressure of one ounce when the axle was running, but required thirty-two pounds to move it when the axle was still.

Another box was moved by four ounces with motion, but required forty pounds without motion of the axle.

A third box under considerable pressure could be moved readily by a pull of six ounces, but fifty pounds would not start it when the axle was still, and indeed, on trial, all the muscular force I could apply to it by my hands, with my foot against the machine, failed to start it.

A spring balance was used in these experiments, for pulling the box in a line parallel to the axle.

Here we employ forces anywhere from 160 to one, up to perhaps 1,000 to one, for moving the same body, under the same load and conditions, except that of the revolving or standing shaft beneath it.

This phenomenon of friction proved a marvel to all who witnessed it. The temptation was great to theorize upon the extraordinary performance, but no theory was offered in explanation of it. A practical suggestion was made, however, in reference to planer-bed motions, and the like, which drag so heavily upon their fixed ways. If, as then proposed, revolving shafts were placed in the bed-ways, and the table fitted to them, a pound pressure would move the table and its load and forth on the revolving ways, where 1,000 pounds or more would be required to do this work upon the usual fixed V's of planers, as they are generally built.

Numerous applications of this principle will readily suggest themselves to the ingenious reader, when considering the necessity and the difficulties of moving dead loads, especially where ease and celerity of the movement may be required.

To the writer, this unique action, as if the loaded box were floating, was an instructive object-lesson in mechanics.

PROCEEDINGS  
OF THE  
CHEMICAL SECTION  
OF THE  
FRANKLIN INSTITUTE.

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*[Stated meeting, held at the Institute, Tuesday, April 21, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, April 21, 1891.

Dr. WM. H. WAHL, President, in the chair.

Twenty-two members and three visitors were present.

Mr. Geo. M. Beringer was proposed for membership, and at the close of the meeting he was elected a member of the Section by the committee on admissions.

Prof. Joseph W. Richards read a highly interesting paper, entitled "On the Heats of Formation of Metallic Fluorides." It was referred for publication.

Mr. Reuben Haines followed with a paper, entitled "On the Ammonia Process in Water Analysis;" this was also referred for publication.

Mr. Haines presented another paper giving the results of an analysis of an artesian well water from Germantown; it was read by title and referred for publication.

Mr. Fred. E. Ives called the attention of the Section to a letter from Prof. Chas. F. Himes, discussing the experimental results and the theoretical explanation of Prof. Lippmann in the matter of photographing in colors. He also read a portion of a paper recently published by Capt. Abney, and bearing on the same subject.

Dr. Wahl, on behalf of Mr. John Carbutt, presented a communication on the "Diazotype Process of Photographic Dyeing and Printing." Several highly satisfactory specimens of results accomplished with this process were submitted for inspection by the members; they were regarded with much interest.

The Section then adjourned.

WM. C. DAY, *Secretary*.



ON LOCAL STANDARDS IN WATER ANALYSIS  
WITH SPECIAL REFERENCE TO PHILADEL-  
PHIA AND ITS VICINITY.

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BY REUBEN HAINES.

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[*Read at stated meeting of the Section, held February 17, 1891.*]

One of the difficulties presented in the "Analysis of potable water" is the obtaining of a suitable standard with which to compare the results of analysis of any given water, in order to form a correct judgment of its character for drinking purposes.

A general standard, such as is given in some published works on this subject, is found in many cases inapplicable or misleading. Special local standards are therefore advised for each locality. These may be obtained by collecting a considerable number of analyses of such well and spring waters of a given district as are, as far as possible, in a natural uncontaminated condition, and an average derived from these may form a standard of purity, to which a potable water should conform quite closely.

It is true, however, that one should not apply such a standard as a "hard and fast rule" to *all* waters in the particular locality for which the standard is devised. Different classes of waters should be judged by separate standards and the results of analyses of waters of different classes should be tabulated separately. Thus river waters should not be compared with well waters. They should be judged by somewhat different standards. Lake or pond waters may be judged by river-water standards under ordinary conditions of location and surrounding drainage area. Well waters, located within the same geological district, may be compared with each other. Yet the writer fully coincides with the statement of the late Prof. Wm. R. Nichols that "there always will be difficulty in deciding how near to any limit any suspicious water may come and still be used with a reasonable degree of safety;" and that

"every doubtful water must be considered by itself with all the light that can be brought to bear upon it."\* Strictly speaking, a pure drinking water should be only one which is altogether above *any* suspicion of contamination with sewage or other injurious substances. But, it may not be practicable to obtain such absolutely pure natural water in some localities, as, for instance, in a village having no public supply. The question then arises, what constitutes a reasonably pure water under the special circumstances, and to what degree may the evidence of slight contamination through the occupations of man or beast be disregarded without risk of imperilling the health in any way, either at the present time or in the somewhat undefined future; for what may be safe now may not be quite safe a year hence. It is, therefore, highly desirable to become thoroughly acquainted with the locality, by personal inspection of the premises, and to confirm or modify the opinion derived from analysis by the facts observed. In many cases it is difficult, and in some impossible, to form any correct judgment at all without examination of the immediate vicinity of the well. It will not do, in all cases, to depend upon getting the desired information at second hand by correspondence or otherwise.

Nevertheless, a standard of some kind is desirable, in order to secure as much uniformity of practice as possible, and it will be found helpful to have special standards of the kind proposed.

In using such a standard it should be remembered, also, that the judgment is formed partly from the ratio one "element," so to speak, in the analysis bears to another, and partly from the consensus of all the facts, including some which may not be capable of being tabulated in the report.

Knowing of no published standard for well and spring water of the vicinity of Philadelphia, the writer ventures to give the results of some analyses which he has made, during the past fourteen years, of waters of this class, which he

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\* "Water Supply—Chemical and Sanitary," Wm. Ripley Nichols, p. 40-41.

believes both from analysis and from personal inspection of the premises, to be entirely pure and uncontaminated. He submits them as a contribution towards a definite standard for the vicinity of Philadelphia, on the Pennsylvania side of the Delaware River, with the hope that other chemists practically interested in this subject may likewise add the results of their experience, as the writer believes the aggregate number of analyses presented in this paper is too small to establish definitely the proposed standard for such a large area as these cover.

It should be further stated that in many of the following analyses other determinations were made, such as oxygen required to oxidize organic matter by potassium permanganate in place of sulphuric acid. As, however, at first, the Tidy form of this process and later the Kabel method, as modified by Leeds were used, the results of which processes are different and not comparable with each other, all results by the permanganate process have been omitted from the following tabular statement. It is desirable that in subsequent work this determination should be included.

The nitrogen as nitrates was in the earlier analyses determined by Williams' copper-zinc couple method, and in the latter ones by the phenol sulphonic acid method. I obtained excellent results in test analyses by the Williams method with solutions of potassium nitrate, provided the amount of the nitric acid in the solution was not very greatly in excess of the amounts usually found in highly contaminated drinking waters. In the presence of very large amounts of "free ammonia" in well waters, it is often almost impossible to get constant results, but in these cases the quantitative determination may safely be omitted as unnecessary for the purpose of sanitary judgment of the water; a qualitative test would, in that case, usually be quite sufficient. Yet in some cases much free ammonia may be accompanied by but little nitrate. The phenol sulphonic acid method was also tested against the Williams method and a known solution of nitrate, in my laboratory, and these two methods were found to give essentially identical results. There appears, however, according to Mr.

Bartram, to be some reason for questioning the accuracy of the phenol sulphonic acid method in presence of much chloride.

Neither of these methods, nor indeed, any colorimetric test is suitable for solutions containing comparatively very large percentage of nitric acid, such as organic liquids rich in nitrates.

In a number of the earlier analyses, a simple qualitative test for nitric acid was thought sufficient. The test used was the well known ferrous sulphate solution, with equal volumes of pure concentrated sulphuric acid and the water to be examined. This qualitative method was used by Prof. Nichols in his analytical work for the Massachusetts State Board of Health. Tests by the present writer with this solution showed that with careful manipulation it would indicate clearly about one part of nitrogen as nitric acid in 100,000 parts of water, and that it would show a faint reaction with 0.7 to 0.8 parts per 100,000. The test cannot be made a quantitative one, however, except in the sense above indicated. By concentration of the water by evaporation, one can secure a more distinct reaction, but it is very difficult at least, if not impossible, to get the depth of tint to follow exactly in accordance with the degree of concentration of the water, even when closely adhering to an identical mode of manipulation. The sulphuric acid used was always, of course, examined for nitric acid, and rejected if its presence was shown by this test.

Equal measured quantities of the acid and the water to be examined about 5 cc. of each were put into a test tube; the water first and the acid poured in gradually along the sides of the *inclined* test-tube so as to form a layer at the bottom without mixing much with the water. After cooling, a few drops of concentrated ferrous sulphate solution were added and the tube again plunged in cold water. If a colored ring formed at the junction of the acid and water, after slight agitation, the water was considered suspicious of contamination with sewage. If the ring of color was formed immediately and of strong tint, it indicated *dangerous* contamination, and the water was generally found to be

foul in respect to the other parts of the analysis. There are some cases, however, occurring occasionally in which the judgment rendered must be based *chiefly* upon the amount of nitrates present. It is, therefore, better to use a method which can be made quantitative.

In the following tables an *asterisk* (\*) in the column of nitrogen as nitrates indicates that by the ferrous sulphate test either there was no reaction or only the faintest coloration after standing for about half an hour, showing less than one part of nitrogen as nitrate for 100,000 without any concentration of the sample.

The following are the tabulated results of analyses alluded to above, arranged according to the several districts included. All numerical results are stated in parts per 100,000.

## GERMANTOWN.

No.	Description, Location, etc.	Date.	Free NH <sub>3</sub>	Alt. NH <sub>3</sub>	Cl.	Nitr'g'n in Nitrates	Total Solids.
1	Haines St E. of Chew St. Well, . . . .	1877	'0014	'0034	1'00	—	—
2	" " " " . . . .	1883	'0040	'0040	'80	*	—
3	" " " " . . . .	1886	none	'0036	1'80	'247	11'0
4	" " " Spring, . . .	1877	'0012	'0030	—	—	—
5	" " " same " . . . .	1884	'0012	'0034	1'00	*	—
6	" " " " " . . . .	1889	'0010	'0052	'80	—	10'5
7	" " " " " . . . .	1889	none	'0030	'80	*	—
8	High Street Well, . . . . .	1877	'0026	'0064	1'40	*	5'0
9	Mill Street, E. of Willow, Well, . . . .	1880	'0022	'0062	1'80	*	22'8
10	Clapier Street Spring, . . . . .	1889	'0010	'0052	1'60	'720	13'0
11	West Cheltenham Avenue Well, . . . . .	1877	'0040	'0052	1'00	*	8'5
12	Wayne Junction Well, . . . . .	1882	'0026	'0050	1'80	'520	—
	Average, . . . . .	—	'0018	'0045	1'15	'495	11'8

## VICINITY OF FRANKFORD.

13	Well, . . . . .	1886	'0040	'0030	1'10	'741	10'5
14	Spring in Woods, . . . . .	1888	none	'0030	.80	'257	9'0
	Average, . . . . .	—	'0020	'0030	'95	'499	9'7



## BRANCHTOWN TO CHELTEN HILLS.

No.	Description, Location, etc.	Date.	Free NH <sub>3</sub>	Alt. NH <sub>3</sub>	Cl.	Nitr'g'n in Nitrates	Total Solids.
15	York Road Well, . . . . .	1890	none	'0040	1'20	'150	10'6
16	" " . . . . .	1881	'0010	'0040	2'00	*	7'5
17	Washington Lane Well, . . . . .	1886	'0010	'0050	1'00	'412	10'5
18	" " same Well, . . . . .	1889	none	'0050	'80	'370	9'0
	Average, . . . . .	—	'0005	'0045	1'25	'311	9'2

## HAVERFORD AND BRYN MAWR.

19	Vicinity of Hav. College Well, . . . . .	1880	'0010	'0056	1'10	*	12'9
20	" " " . . . . .	1889	'0048	'0046	'70	'600	9'0
21	" " Spring, . . . . .	1891	'0020	'0070	1'00	'750	11'5
22	Bryn Mawr Spring, . . . . .	1880	'0022	'0070	'70	*	5'7
23	" same Spring, . . . . .	1881	'0010	'0030	'70	*	5'0
24	" " . . . . .	1883	'0026	'0066	'60	'263	4'5
25	" Well, . . . . .	1880	'0040	'0050	1'10	*	10'0
26	" " . . . . .	1883	'0010	'0038	1'20	'347	8'5
27	" " . . . . .	1888	'0026	'0030	'70	'412	7'4
	Average, . . . . .	—	'0023	'0051	'87	'474	8'3

## OVERBROOK.

28	East of Penna. R. R. Well, . . . . .	1882	'0046	'0040	1'45	*	12'0
29	West of " " Spring, . . . . .	1887	'0040	'0030	'70	'249	10'5
	Average, . . . . .	—	'0043	'0035	1'07	'249	11'2

## SPRING MILL NEAR CONSHOHOCKEN.

30	Public Spring, hardness 12°2, . . . . .	1889	'0040	'0050	'50	'370	16'5
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## ARTESIAN WELLS.

31	Germantown, 312 feet deep, rock struck 8 ft. from surface, . . . . .	1877	'0020	'0040	3'00	*	30'0
32	Columbia Ave. near Broad St., . . . . .	1886	'0160	'0030	1'50	'135	28'0
33	North Penn. R. R. near City, . . . . .	1890	'0044	'0030	1'10	'100	15'0
34	Vicinity of Darby, Delaware Co., Pa., .	1889	'0010	'0050	1'10	'470	—

It is thought proper to place the above artesian wells in a separate class by themselves, and they are not therefore included in the following computation of averages for the above tabular statements. The artesian wells show plainly the degree of purity obtainable by them. Numbers 32, 33 and 34 are 100 feet in depth, or but slightly more than that. In No. 31 the water rises to about thirteen feet from the surface, and an iron tube extends nearly the whole distance through rather soft rock. Since the above analysis it has become impure, and is seldom used for steam purposes.

The following are the averages of the total figures in the above thirty analyses of shallow well waters, these being calculated only from the actual numerical results in each column, the qualitative tests as well as the blanks being omitted in the computation.

	<i>Parts in 100,000.</i>
Free ammonia, . . . . .	'0020
Albuminoid ammonia, . . . . .	'0045
Chlorine, . . . . .	1'07
Nitrogen in nitrates, . . . . .	'427
Total solids at 100° C., . . . . .	9'60

I ought to say that the figure for nitrogen as nitrates is not quite satisfactory, for it represents only one-half of the whole number of analyses, and assumes that the other half if determined quantitatively would yield about the same average which may not be strictly correct.

The determination of total solids is of not much importance, and is of use chiefly in this connection to show the general character of the water as regards hardness. It is to be noted that most of these well waters are quite soft. In hard sulphate waters the natural amount of chlorine appears to be higher (about double) than in softer waters. In the usual course of analysis the determination of total solids gives useful information in several ways.

In conclusion, the above average results is submitted as an approximate standard for the purest available shallow well waters of this district to be corrected by additional analyses.

As a necessary part of this work it is highly desirable to obtain figures representing the *highest limits* for waters of

this class under *specified conditions*. This will be more difficult to fix definitely, for a certain latitude must always be permitted to satisfy special conditions. This limit will certainly be different for different sections of the country. I think, however, that for the vicinity of Philadelphia on the Pennsylvania side of the Delaware River the figures for free and albuminoid ammonia will be found closely to approximate those given by Wanklyn in his book on water analysis. In certain parts of New Jersey, for instance, the limit is probably higher.

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“ON A SOURCE OF ERROR IN THE DETERMINATION  
OF NITRATES IN WATER BY THE PHENOL  
SULPHONIC ACID METHOD.”

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BY GEORGE H. BARTRAM.

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[*Read at the stated meeting of the Chemical Section, March 17, 1891.*]

Owing to certain discrepancies in duplicate estimations of nitrates, by the phenol sulphonic acid process, I was requested by Dr. Samuel C. Hooker to make a series of experiments to ascertain under what conditions this process, which had hitherto been considered reliable, gave these varying results. The work was performed in Dr. Hooker's laboratory and under his direction.

As the principal result of these experiments it was found that phenol sulphonic acid undergoes a change shortly after preparation, as shown by the fact that chlorides, which have no effect on the determinations when made with freshly prepared phenol sulphonic acid, bring about low figures when the acid has been kept for some time before use.

The disturbing action of the chlorine is already well marked, when the quantity present in a given volume of water is about four times as great as the nitrogen of the nitrates. Correct figures were, however, obtained even when the phenol sulphonic acid had been kept eighteen months, with water in which the chlorine did not much exceed the

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nitrogen of the nitrates. Water containing twenty parts chlorine to '5 parts nitrogen gave the following decreasing quantities of N as the age of the solution increased:

<i>N. present.</i>	<i>N. found.</i>	<i>Age of phenol sulphonic acid.</i>
'5	'5	Freshly prepared.
'5	'5	1 day old.
'5	'45	2 days "
'5	'42	13 " "
'5	'40	17 " "
'5	'40	27 " "

Water containing ten parts chlorine to '5 parts nitrogen gave a similar series of figures:

<i>N. present.</i>	<i>N. found.</i>	<i>Age of phenol sulphonic acid.</i>
'5	'5	Freshly prepared.
'5	'5	2 days.
'5	'42	13 " "
'5	'40	17 " "

With water containing two parts chlorine to '5 part nitrogen the results similarly fell off.

<i>N. present.</i>	<i>N. found.</i>	<i>Age of phenol sulphonic acid.</i>
'5	'50	Freshly prepared.
'5	'50	13 days.
'5	'40	27 " "

The above determinations were made in duplicate and the standards were always freshly prepared.

A solution of phenol sulphonic acid two and a half months old gave, as the mean of four determinations, '30 instead of '5 N, in the water containing ten parts chlorine to '5 N.

A solution eighteen months old also gave '30 N for the same water.

By the removal of the chlorine correct results were obtained.

The above experiments show that in the presence of relatively large quantities of chlorine twenty per cent of the nitrates, or in extreme cases even as much as forty per cent. may fail to be recorded by the use of this process, unless the precaution be taken to use freshly prepared phenol sulphonic acid, or to remove the chlorine.

## ANALYSIS OF A CHROMITE.

BY H. PEMBERTON, JR.

[Read at the meeting of the Chemical Section, held March 17, 1891.]

On the Pacific Coast of the United States there are several deposits of chromite. As no complete analysis of any of these minerals has, to my knowledge, been published, it may be of interest to place on record an analysis I made some time ago.

According to Wm. Irelan, Jr., State Mineralogist of California, throughout the Santa Lucia Mountains and the coast hills of San Luis Obispo County, Cal., are found serpentine rocks with beds of chromite, in greater or less masses existing as loose and fragmentary rocks in the ravines and on the hillsides, and as pockets and veins on the mountains. One of the best mines of the neighborhood is the Pick and Shovel, located on the South Fork of Chorro Creek, at an elevation of 1,800 feet.

A sample of the chromite from this mine, selected as free from gangue as possible, gave on analysis the following results:

$\text{Cr}_2\text{O}_3$ , . . . . .	52.68
$\text{Al}_2\text{O}_3$ , . . . . .	11.40
$\text{Fe}_2\text{O}_3$ , . . . . .	3.52
$\text{MgO}$ , . . . . .	16.23
$\text{FeO}$ , . . . . .	11.77
$\text{MnO}$ , . . . . .	0.15
$\text{SiO}_2$ , . . . . .	3.40
$\text{H}_2\text{O}$ , . . . . .	0.94
	<hr/>
	100.09.

An analysis of the gangue showed it to be serpentine, containing a trace only of ferrous oxide. No other foreign mineral could be detected on a careful examination. Deducting, therefore, the water, silica, and quantity of magnesia (3.26 per cent.) combined with the silica (as serpentine)



and calculating the remainder to 100 parts, we obtain for the pure mineral:

Cr <sub>2</sub> O <sub>3</sub> , . . . . .	56.96	.374	} = .519
Al <sub>2</sub> O <sub>3</sub> , . . . . .	12.32	.121	
Fe <sub>2</sub> O <sub>3</sub> , . . . . .	3.81	.024	
MgO, . . . . .	14.02	.350	} = .529
FeO, . . . . .	12.73	.177	
MnO, . . . . .	0.16	.002	
	<hr/>		
	100.00		

$$R_2O_3 : RO = 519 : 529 = 1.00 : 1.02.$$

The mineral was decomposed by fusion with sodium carbonate. (Christomanos' process slightly modified.) The mixture of ore and Na<sub>2</sub>CO<sub>3</sub> is heated over night by a Bunsen burner, the crucible being loosely covered by its lid. Next morning it is heated for one hour over the blast lamp, with frequent stirring. The process has the advantage of giving complete decomposition of the mineral, with the introduction of no bases other than the alkalies.

The ferrous oxide was determined by solution in sulphuric acid in a closed tube, under pressure. According to Francis C. Phillips (*Fres. Zeitschrift*, **12**, 189) 1.34 is the specific gravity of the acid that is most favorable to the solution of chromite in this manner.

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## AN APPARATUS FOR HEATING SUBSTANCES IN GLASS TUBES UNDER PRESSURE.

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BY H. PEMBERTON, JR.

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[Read at the meeting of the Chemical Section, held March 17, 1891.]

Chemists who do not happen to have in their laboratories oil or air baths for heating closed tubes, can make an air bath at short notice, from materials furnished by all dealers in steam fittings.

*Order :*

(1) One four-inch wrought-iron pipe, eighteen inches out to out, with usual thread on each end. At about nine

inches from either end this pipe is drilled and tapped for a one-inch nipple, in such a manner that a pipe introduced would pass, not on a line with the radius, but about half-way between the axis of the four-inch pipe and its walls; in other words, it would be on a line with a chord of the circle.

(2) One one-inch wrought-iron nipple, two inches long, one-inch thread on one end.

(3) Two four-inch malleable iron caps, drilled and tapped for a one-inch pipe.

(4) One one-inch wrought-iron pipe, twenty-four inches out to out, with a three-inch straight thread on each end.

(5) Two one-inch iron caps. A hole, one-eighth of an inch in diameter, is drilled in the end of one of these caps.

The above order can be given *literatim*, and will be understood by the dealer, who will furnish, at a trifling cost, the materials, cut and tapped as ordered.

*Fig. 1* shows how the whole is put together. The numbers on the figure correspond also to the numbers of the paragraphs of the order as given above.

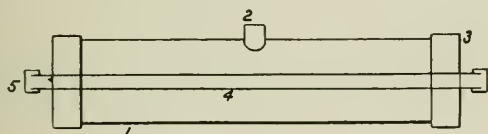


FIG. 1.



FIG. 2.

*Fig. 2* is an end section. A cork is inserted in 2 and through it a thermometer, the bulb of which is on a level with the interior pipe. The whole is supported on a few bricks at either end, and is kept steady and in place by a couple of weights or half bricks. It is heated by one or two Bunsen burners, according to the temperature desired.

PROCEEDINGS  
OF THE  
ELECTRICAL SECTION,  
OF THE  
FRANKLIN INSTITUTE.

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*[Proceedings of the stated meeting of the Electrical Section, held Tuesday, April 7, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, April 7, 1891.

Prof. EDWIN J. HOUSTON, President, in the chair.

Present, thirty-four members and visitors.

The minutes of the previous meeting were read and approved.

The treasurer presented bills for stationery, printing and clerical work, which were ordered paid when approved.

Twelve nominations to membership were referred to the committee on admissions.

At the request of the Secretary of the Institute, the subject of re-wiring the building in accordance with best modern practice, with a suggestion that detailed specifications, be prepared by the Section for that purpose, was presented and referred to a committee consisting of Messrs. Pike, Billberg and C. Hering, to consider and report.

Prof. C. Hanford Henderson presented a communication on "A Suggestion in Arc Lighting." The speaker called attention to the fact that progress in illumination has consisted in the substitution of continuous for reservoir systems. While the two essential factors, the source of heat and the substance to be made luminous, are combined in the candle, oil and gas as ordinarily used, they are separated in incandescent gas and electric lighting. In arc lighting the supply of energy is continuous, but the light-yielding substance, carbon, must be frequently replenished. The question as to whether arc lighting could be made continuous had therefore presented itself, and an experimental attempt was made to use gas issuing from hollow wrought-iron terminals, in place of the solid carbon. It was found that the gas and the arc formed two mutually repellent crescents of light; the terminals were fused and one of the gas jets was closed up. This negative result not being thought conclusive, the suggestion was brought before the Section in the hope of discussing the question as to whether the problem was sufficiently promising to merit further attention.

Prof. Houston stated in discussion that hollow carbon electrodes had been used at different times since 1852, fed by hydrocarbon oil, and by gas containing pulverized carbon, and the like, but without success, owing to the fusion of the terminals. As the intensity of the light varies with the sixth

power of the temperature, he considered a continuous arc impossible with our present refractory materials. There was further discussion by Messrs. Pike, C. Hering and Wahl.

Prof. L. F. Rondinella read a paper on "An Experimental Analogue for Direction of Induced Currents," which was discussed and referred for publication.

Prof. H. S. Hering gave the result of continued experiment on "The Effect of Atmospheric Pressure on Batteries," which seemed to show that the E. M. F. of standard cells did not become constant until about two hours after setting up. This result elicited considerable discussion.

Upon motion of Mr. C. Hering, the treasurer was instructed to purchase a letter-box for the reception of queries on electrical subjects, which would be periodically submitted to the Section.

The meeting then adjourned.

L. F. RONDINELLA, *Secretary*.

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## AN EXPERIMENTAL ANALOGUE FOR DIRECTION OF INDUCED CURRENTS.

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BY L. F. RONDINELLA, M.E.

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[*Read at the stated meeting of the Electrical Section, April 7, 1891.*]

The striking analogies which exist between current electricity and magnetism, form an important aid in acquiring a clear understanding of the fundamental principles of modern electrical science. For example, an electric current is assumed to pass through a generator from its south to its north pole, and from thence through an external circuit back to the south pole again. So, also, are magnetic "currents," or lines of force assumed to pass through and outside of a magnet.

In an induced electric current lines of force are made to flow around a conductor in a right-handed or clockwise direction when the observer is looking along the conductor in the direction of the current, or toward the south pole of the generator. Similarly, in an induced- or electro-magnet, an electric current is made to flow around the core of the magnet right-handed when the observer is looking in the direction of the lines of force in the core, or toward the south pole of the magnet.

The directions of lines of force are usually discovered by

the effect produced upon a small permanent magnet or magnetic needle, which will always come to rest with it, north pole pointing in the direction in which they flow through it. Then since the direction of a current depends upon the direction in which the lines of force flow round it, the former also is discovered by noticing the way in which the needle is deflected by the latter.

As there are two directions in which the current may flow through the conductor, and two positions in which the needle may be placed relative to it (above or below), it has been found necessary to use some *memoria technica* to quickly

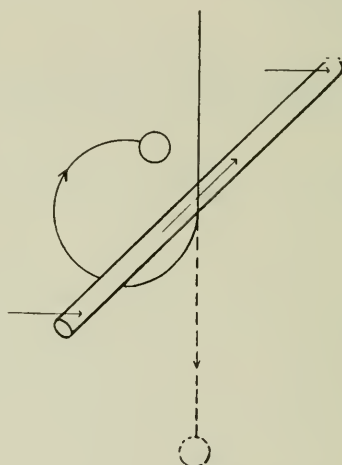


FIG. I.

determine the direction of the current, from the way in which it deflects a needle; and among several devices that have been proposed, the following are most used, although they all present certain difficulties.

Ampère's rule is to imagine an observer swimming with the current in a conductor, with his face always toward the north pole of the needle, which will then be deflected toward his left hand. The disadvantages of this rule are the difficulty of imagining the direction of the observer's left hand when he is placed in unusual positions, and the fact that it was intended for determining the deflection of the needle from the direction of the current which it pre-



supposes to be known, and it must therefore be used inversely for our purpose.

Another device is to remember the fact that when a current flows from South to North Over a needle, its north pole points West, by noticing that the direction initials spell the word SNOW; and similarly a third rule impresses the statement that when a current flows from North Over a needle to South its north pole point East, by spelling the word NOSE with the direction initials. A vital point that seems to be most troublesome to remember in these, is whether the current is over the needle or *vice-versa*, and the difficulty of always finding a part of the conductor running north and south interferes with their direct application.

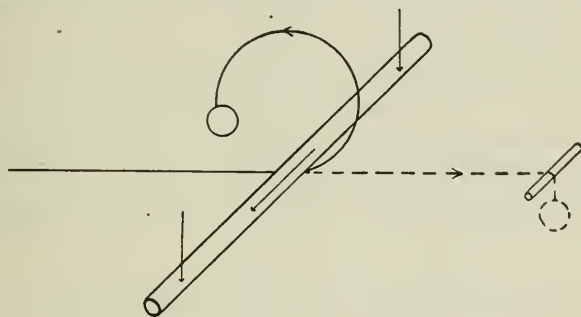


FIG. 2.

None of these devices can be used to predict the direction of the current that will be induced in a conductor when it is moved in a certain direction through a known magnetic field, and for this purpose the only one familiar to the writer is to use the right hand with (*a*) the thumb, (*b*) the first and (*c*) the second finger pointed outward in the three rectangular directions to represent respectively (*a*) the direction in which the conductor is moved, (*b*) the direction of the lines of force, and (*c*) the direction of the induced current. The trouble in applying this is to remember which hand to use and what each finger represents.

As an addition to these, and as an analogy that is not liable to confusion, the following experiment is suggested: If a small weight be suspended at the end of a piece of string (*Fig. 1*), the force of the weight acts downward

in the direction of the string, and the latter may be used to represent a "line of force" acting vertically downward. If then a horizontal rod, which may be used to represent a "conductor," be moved from left to right so as to strike sharply against the string a short distance above the weight, the latter with its "line of force" will revolve around the rod or "conductor" right-handed—a correct analogy to the phenomenon of electric induction. To represent lines of force acting horizontally in either direction, or vertically upward, the string may be held opposite or below a rod over which it is passed (*Fig. 2*), the force of the weight on its end acting along it in the proper direction. The rod may then be moved as desired, and the resulting revolution of the weight and string will remain a correct analogy for electric induction under similar circumstances. Then knowing the direction of the lines of force around the conductor in any case, the direction of the induced current flowing through it is readily determined by remembering that the observer is looking in the direction of the current when the lines of force flow right-handed around the conductor.

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### A NEW FORM OF STANDARD CELL.

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BY CARL HERING.

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[*Read at the stated meeting of the Electrical Section, held Feb. 24, 1891.*]

The standard cells used at present may be said to be limited to the Clark cell and the Daniell cell. Each one of these has its specific advantages and disadvantages. The Clark cell is always ready for use, but is difficult to make, is affected by temperature, and has the important disadvantage that if by accident it is short-circuited it polarizes so rapidly and so much, that it is of no use at all until it has been allowed to rest and recuperate for some time. Such accidental short circuits are not unlikely to occur, and they may furthermore occur, by the crossing of two wires, for instance, without the knowledge of the operator, thus making it possible that the

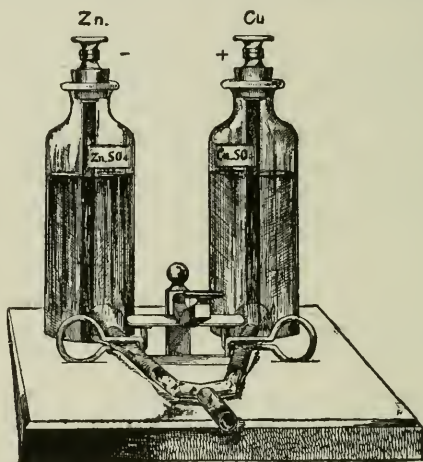
measurements made with it are incorrect. The Daniell cell, on the other hand, does not polarize at all, it may be short-circuited and used almost immediately afterwards; furthermore, it is very easy for any one to construct a Daniell, and it is practically independent of temperature. But it has the important disadvantage that it must be freshly set up just before using and cannot be kept standing. In their good and bad qualities, therefore, the two cells are diametrically different.

The object of the present form of cell is to try to combine all these good points in one and the same cell. As the polarization of the Clark cell is an inherent fault and cannot be remedied, there remains only to endeavor to devise a form of Daniell which is always ready for use. The difficulty is due to the fact that there are two liquids which must be in contact with each other; these liquids will tend to mix, and if they mix to the slightest extent only, the cell can no longer be used as a standard. The difficulty therefore resolves itself to a means for enabling two liquids to be in contact without mixing. Numerous forms of Daniell cells have been devised with this object, but they accomplish it only in a degree, as for instance, by capillary tubes, porous diaphragms, etc., all of which, however, do not prevent mixing, but merely diminish the rapidity of the mixing.

In a number of these forms, in which two bottles or reservoirs are connected together by a tube, the mixing takes place quite rapidly unless the pressure of the liquid is exactly the same in both bottles, a condition not always easily complied with.

Some time ago the writer had occasion to use a cell of this form devised by Prof. Barker, which consisted of two bottles containing the two liquids with their respective electrodes, the bottles being connected at their bottom by a small tube with a stopcock. This cock was opened only during the moment when the cell was being used. But even during a few seconds no small amount of mixing took place, especially if the pressure was different in the two bottles. It is very difficult to avoid the latter, owing to the different specific gravities of the liquids and their different

heights in the two bottles. The writer therefore modified this form by inserting two stopcocks in this tube and allowing the portion of the tube between the two cocks to communicate directly with the air, as shown in the adjoining cut. By this means the liquids, which mix at their contact, are allowed to drain off as rapidly as they mix. As there is an outward flow of the liquids through the cocks, the mixed liquids cannot pass back into the bottles. The liquids in contact with the electrodes will therefore always remain pure. To prevent too rapid a flow, which would needlessly waste the liquids, some filter paper, cotton or asbestos is packed into the tubes. The intermediate tube may be



drained completely, there is always a film of liquid sufficient to answer for a contact if the cell is used on open circuit, which is the only way any standard cell should be used. It was even found that the film of liquid through the cocks when closed, was sufficient to give an electro-motive force which, however, falls as soon as the slightest current is allowed to flow.

The internal resistance of such a cell is naturally quite high, from 10,000 to 20,000 ohms, but this is no disadvantage for the usual class of open-circuit testing.

The e. m. f. of a Daniell cell, according to a report of Dr. Fleming, is as follows: when the specific gravity of the zinc

solution is 1.2, and that of the copper is 1.2, when the zinc is amalgamated, and the copper is freshly electroplated with an untarnished, salmon-colored coating, having no brown spots, the e. m. f. is 1.105 true volts at about 16° C. The temperature correction is negligible. The solutions and metals must be perfectly pure, and the solutions made with distilled water and filtered, being diluted to the required amount after filtration.

Such a cell may be kept standing indefinitely, and is always ready for use. The only remaining objection is that the copper electrode does not remain clean. It should be freshly electroplated, before using, which is readily done in a test tube with a platinum anode. To overcome this, the writer suggests to amalgamate the copper, and when not in use to keep it in a tube of mercury. Whether this introduces other objections has not yet been tested.

Another modification suggested is to place in the bottom of the bottle containing the zinc solution, a quantity of pure zinc scrap, not in contact with the electrode. Similarly, copper scrap in the other bottle. Should any mixture of the liquids take place, they will be freed of their impurities by these metals, which, not being in contact with the electrodes, will not affect the e. m. f.

In experimenting with this cell a curious phenomenon was noticed which does not appear to be known. It was found that the slight pressure produced in the bottles by inserting the corks at the top, produced decided differences in the e. m. f. of the cell; this was noticed for each of the liquids, and was different in the two cases. It was therefore found essential to use a cork with a hole through it, so as to prevent this pressure from being exerted. The effect of this pressure has since been investigated by a very extended and interesting set of experiments made by Prof. Hermann Hering, and will form the subject of a paper to be read by him following this one.



BOOK NOTICES.

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*The Theory and Practice of Surveying.*—Designed for the use of surveyors and engineers generally, but especially for the use of students in engineering. By J. B. Johnson, C.E., Professor of Civil Engineering, in Washington University, St. Louis, Mo., etc. Seventh revised and enlarged edition. 730 pp. New York: John Wiley & Sons.

This work, now so well known, the first edition of which was favorably noticed in this journal so late as January, 1887, has already reached its seventh edition, and now presents some noteworthy additions and other modifications.

A new table of magnetic declinations in North America, published by the Coast Survey in 1890, and a new table for refraction-corrections to be applied to the sun's declination in using the solar compass, are given in place of the former ones; and some new surveying instruments are described and illustrated, including the cross-section polar protractor used in the tunnel for the new Croton aqueduct.

The opening sections of Chapter vii, on Land Surveying, have been re-written, and in doing this greater prominence has been given to the United States public lands' system. An important addition is a series of rules deduced from legal decisions bearing on the re-survey of private lands.

Chapter viii, on Topographical Surveying, is substantially unchanged, except by the addition of a description of Porro's telescope, which reads stadia distances from the vertical axis of the instrument.

A new appendix (E) gives the positions of the base-lines and principal meridians used in laying out the United States public lands; and a new table (xii), condensed from the 1890 *Manual of Instructions*, issued by the General Land Office, gives the necessary data for obtaining the azimuth of *Polaris* at any hour, thus obviating the necessity of waiting for an elongation of the star.

We are glad to note that the very serious lack of an index has been, in a measure, supplied. A very much more complete one is still a great desideratum. T.

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*First Lessons in Metal-Working.* By Alfred G. Compton. pp. vi, 170. New York: John Wiley & Sons. 1890.

The author has prepared this work as a companion volume to "First Lessons in Wood-working," and in it explains the methods of doing forge work, foundry, vise- and other purely hand-work. About one-third of the book is devoted to exercises on the forge, telling in detail the methods to be followed to produce the pieces required. About an equal amount is devoted to the production and properties of cast iron, wrought iron and steel. Foundry work is scantily treated of in seven pages and almost as many erroneous ideas can be gathered for this part as there are pages. In the portion devoted to chipping and filing, the instruction is defective, in that the part

played by the hands and arm of the workman are forgotten. The rest of the book is given to drilling, hack-sawing and soldering.

A judicious selection of American names, instead of foreign ones, would have taken away much of the flavor of having been worked up from text books instead of actual practice, and perhaps would have given the students the idea that, for instance, files are made in this country and that iron is made in a few places in the United States.

Covering so much ground in a few pages particular care should have been taken that the statements made and the direction given were exactly correct, but there is in some parts of the work a looseness of statement that is sure to convey incorrect ideas.

The author has in many cases considered that one view (not isometric) gives sufficient information as to the shape of the piece. This is unfortunate, as in no other places are correct working drawings of so much use as in teaching elementary work of this character, and working to drawings properly made adds nothing to the labor of making a piece, but gives the student a better idea of what working drawings are for than can be obtained in any other way.

H. W. S.

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*Constructive Steam Engineering.* By J. Jay M. Whitham, M.E., C.E. pp. v, 909. New York: John Wiley & Sons. 1891.

The author in this volume has covered the subject of steam engineering, as far as the appliances go, more completely than has ever before been attempted. There is hardly a single branch of the subject, of which numerous examples are not given. While, of necessity, trade circulars have been the principal sources of his figures, the grouping together of all the information obtainable on any one subject will be of great convenience to those desiring extended knowledge of what appliances are obtainable, without having to refer to innumerable pamphlets.

Something of the theory is added, which, however, adds little to the value of the work, as those to whom the work will be of most service have probably all that they desire in that line.

\* It is well printed, well indexed, and altogether is a desirable work to have.

H. W. S.

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*Mécanique Professionnelle, Outils, Organes, Machines Élémentaires.* By Ch. Casalunga. Part 1. Tools. Office of the *Chronique Industrielle*. pp 103 and 10 plates. Paris. 1890.

This work is the first part of a series of ten, each of which is to be devoted to one branch or specialty of construction. There are three general divisions in this volume, the first devoted to the metals employed in construction, the second to hand tools, and the third to some of the machine tools.

That portion relating to the metals employed gives generally the physical characteristics of wrought and cast iron, steel, copper, bronze or brass, and phosphor-bronze. This is followed by a series of notes on all sorts of sub-

jects relating to the metals:—as homogeneous iron, prevention of corrosion, prevention of crystallization, bronzing and leading iron, preservation by painting with various substances, case-hardening, removing rust, forging steel, manganese and chrome steel, etc., testing machines, manganese bronze, delta metal, aluminium bronze, etc. This is followed by a bibliography, covering references to French journals for the past forty years, and this is followed by the advertisements of the dealers in the various metals and current prices for February, 1889. Under the head of hand tools are files, chisels, hammers, drills, reamers, taps and dies, milling cutters, wrenches and handles. This part is also followed by numerous notes such as recutting files by sand jet, Elliott's cutting-off tool, Stephen's vises, etc.; which is again followed by bibliography and advertisements and price-list. The portion relating to machine tools is divided into such as are used for iron, including drilling, boring, planing, shaping and milling machinery; machine tools for wood, covering sawing and planing machines, and machine tools for forging. Among the notes, radial drills, portable drills, the "Stow" flexible shafting, emery wheels, multiple drills, special tools for facing nuts, band saws, etc., are considered. This part also ends with a bibliography, advertisements and current rates in October, 1889. The plates give tables of dimensions for taps, tap wrenches, end, box, pin and S wrenches, adjustable pieces for turning an ordinary face plate into a chuck and sockets for the drill press, all, of course, in centimetres.

H. W. S.

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*The Mechanics of Engineering and of Machinery.* By Dr. Julius Weisbach, vol. 3, part 1, section ii. "The Machinery of Transmission and Governors," revised and enlarged by Gustav Herrmann; translated by J. F. Klein, D. E. 8vo, pp. vi, and from 535 to 1084. New York: John Wiley & Sons. 1890.

Of the many works on the machinery of transmission, but few are as yet available to the English-speaking engineer. The greater part of the literature on this subject is written in the German and French languages and of this but little has been translated into English. This volume will, undoubtedly, prove of great value to the American engineer and more especially to the engineering student, being a complete theoretical investigation into this subject. The mathematical methods used in the earlier volumes of this work, which were of a most indirect character and which have to a great extent caused vol. 1, on the "Strength of Materials," "Hydraulics," etc., to be almost entirely replaced by other text books in our technical colleges, are abandoned in this volume, the methods of calculus being used where the deductions are thereby facilitated. On the other hand, the work is hardly one which could readily be used by the practical man, since the results are not arranged in such a way as to be easily accessible. For this purpose, *Der Konstrukteur*, which at present is being published in English in the columns of *Mechanics*, will be preferable. For those, however, who wish to acquire a theoretical knowledge of the machinery of transmission, the book may be highly recommended. The first chapter, which is on "Flexible Trans-

mitters," contains a discussion on the considerations which enter into the manufacture of hemp and wire ropes, chains, hooks and other accessories. Formulæ showing the relative value of these organs for lifting are deduced, the transmission of power by ropes and chains having been considered in a previous volume. In connection with this subject are given a few tables, showing the relative strength, weight and cost of different ropes and chains, from which it is evident that in all cases wire rope is the lightest and most economical.

Screws and screw wheels are next discussed. The functions of screws and the advantages of different types for different purposes are clearly set forth. The forces required to produce others and to overcome the frictional resistances and the corresponding efficiencies are established for different forms of threads. A few tables giving the proportions of threads for different systems would not be out of place here. Considerable space is devoted to crank trains. Graphical methods of finding the motions and velocities in different parts of these mechanisms are clearly explained. There are also numerous descriptions and illustrations of various mechanisms of this class, and their practical applications are given. Under this head, the action of the weight and velocity of the reciprocating parts of the steam engine has also received a thorough discussion.

Chapters vii and viii on Cam Trains, Engaging and Disengaging Gears, give a description of the various forms of clutches, ratchet motions, escape-ments, etc., and of the theories underlying the same, with numerous illustrations of their practical application.

The last chapter is on Regulators, and treats in this connection of the various forms of brakes, the fly-wheels and governors, with analytical and graphical explanations of their action. Finally, there has been added by Prof. Herrmann an appendix on the graphical statics of mechanisms. In this the supporting surfaces of mechanisms are replaced by a reacting force, which would act in a certain direction were there no frictional resistances. For the latter a correction is made in the direction of the reacting force. This principle is applied to a number of cases occurring in practice, and proves to be a simple and efficient method of determining the forces in mechanisms.

E. R. K.

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*Cosmic Law of Thermal Repulsion.* An essay suggested by the projection of a comet's tail. New York: J. Wiley & Sons. 1889.

In reading that recent anonymous little essay, on "The Cosmic Law of Thermal Repulsion," one is especially struck with the novelty of the idea on the one side together with a desire to base all argument and reasoning upon the most recent conventions of science, while one of the most fundamental notions of modern physics has been entirely overlooked.

From this point of view the book seems to be somewhat unbalanced. Whether this condition of things exists because the point of support does not bear the necessary equilibrium relation to the centre of gravity, or whether this latter point has been lost sight of in the "conjugal antagonism of heat



and gravity" might be a subject for further debate. In Section 2 on "General Principles," the writer seems to have ignored entirely the sharp distinction which is now insisted upon in science between force and energy. While a correct definition of force is implied at the outset, almost immediately several forms of energy are placed in the list of forces; *e. g.*, "gravitation and heat" are mentioned in the same clause as ever-existing *forces* when matter is present, while in the next sentence the *force exerted by heat* is referred to. It seems unfortunate then that in a paper of this sort the now universally accepted classification of nature into *matter* and *energy* should not have been adopted.

We have in science the most important generalizations which are called the laws of conservation in the case of both matter and energy, and the truth of which is necessarily assumed throughout the essay, but who ever speaks now of the law of the "conservation of force," unless one reads it in some antiquated text book? Again, on the top of the eighth page is found the statement that expansion "is proportional (with certain well-known exceptions) to the amount of heat absorbed by the body." Now this is not only in general not the truth, but in no single known instance is it true; in other words, the exceptions embrace every known case. The reason is obvious. If heat be applied to a rod for example, it is expended in doing two things, *viz*: expanding the rod against external forces, *i. e.*, doing external work, and in raising the temperature of the body; but this rise in temperature, and therefore the part of the heat applied required for this purpose, is dependent upon the specific heat of the substance which always varies with the temperature.

It would seem that at the beginning of Section 9 would be a suitable place for inserting the clause "(with certain well-known exceptions)" as the expansion is really equal in all directions only in isotropic substances, which are comparatively few.

On the whole, however, it should be said in fairness that the essay seems extremely well written and to show much thought in its preparation, as also a wide acquaintance with the works of many well-known writers.

The interest is held throughout by many amusing illustrations, of which the little fish story of Section 4 ought especially to be noticed. The ideas of "thermal repulsion," however, as well as the general conclusions arrived at, are so novel, that it is to be feared they will not immediately take a place among the generally accepted theories of science, but as to what may ultimately be the result of such speculations, we would not like to venture an opinion.

A. W. G.

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*Formulae and Tables Used in the Application of the Method of Least Squares.*

By Malverd A. Howe. Terre Haute: Moore & Langen. 1890.

Of the thirty-two pages of this little book, nineteen are devoted to the formulæ of the theory of probabilities and of the method of least squares, the rest to a set of tables giving the values of the different expressions used in the applications of this theory. It forms altogether a very convenient book of reference for those whose work requires the use of these important methods. The formulæ are clearly and distinctly arranged and can readily



be used even by those who have little or no knowledge of the theory of the subject.

The tables are eleven in number :

I gives

$$\frac{2}{\sqrt{\pi}} \int_0^{hx} \frac{d(hx)}{e^{\frac{h^2 x^2}{2}}}$$

to three decimal places, for the argument  $hx$ .

II gives the same quantity to seven decimal places.

III gives

$$\frac{2}{\sqrt{\pi}} \int_0^{0.47694 \frac{x}{\gamma}} \frac{d(hx)}{e^{\frac{h^2 x^2}{2}}}$$

to five decimal places for the argument  $\frac{x}{\gamma}$

IV and V give  $0.6745 \frac{1}{\sqrt{n-1}}$  and  $0.6745 \frac{1}{\sqrt{n(n-1)}}$

respectively, to 4 decimal places.

VI and VII give  $0.8453 \frac{1}{\sqrt{n(n-1)}}$  and  $0.8453 \frac{1}{n\sqrt{n-1}}$

respectively, to four decimal places.

VIII and IX give the value of  $a^2$  for  $q=1$  and  $q=2$ , respectively, for use in "Pierce's Criterion," for the arguments  $m$  and  $n$ .

X gives  $n \times 0.675$  and XI gives

$$\frac{1}{e^{\frac{1}{2}}}$$

*Logarithmic Tables.* By George W. Jones. Ithaca: Author. 1891.

This collection of tables consists of seven parts: I, A table of six place logarithms of numbers; II, Physical and mathematical constants; III, Sines and tangents of small angles; IV, Trigonometric functions, both natural and logarithmic; V, Napierian logarithms of numbers from 1 to 1079; VI, Meridional parts; VII, Addition—subtraction logarithms.

The typography and the arrangement of the page, which are points of the greatest importance in a book of this kind, are both excellent. We would call attention particularly to Tables II, V and VII, as being very valuable additions to the other more frequently used tables. The collection of constants in Table II is unusually complete. In Table IV the natural and logarithmic functions are arranged in parallel columns. The *sin.*, *cos.*, *tan.* and *cot.* are given for every minute of arc from  $0^\circ$  to  $180^\circ$ . The principal

defect in the tables is found here, in the absence of tables of proportional parts for making the corrections for seconds.

Appended to the tables is a short description of the mode of using them.

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*Treatise on Trigonometry.* By Oliver, Wait and Jones. Ithaca. 1890.

The most striking features of this text book are the use of the methods of projection in the proofs of the principal formulæ and the addition of much matter, mainly in the way of applications to surveying, navigation and astronomy, not generally found in such text books.

The subject is introduced by some paragraphs upon the method of coördinates in determining the position of a point in a plane, and the principles of directed lines and projections: Then follow the methods for the measurement of angular magnitude and the definitions of the trigonometric functions for the general angle. These and the other principles throughout the book are illustrated by numerous exercises and examples. The different parts of the subject are then discussed in the usual order, the general formulæ and the solution of triangles, right and oblique, followed by the formulæ of spherical trigonometry and the solution of spherical triangles.

The development of the subject from the principles laid down at the beginning is clear and logical, and we certainly have no fault to find with it on that ground. We may be allowed to express a doubt, however, whether the general plan here adopted is the best form in which to present the subject to the mind of the average beginner.

Besides the portions ordinarily given in a beginner's course, sections are given upon the functions of multiple angles, inscribed, escribed and circumscribed circles, areas and projections of polygons, the derivatives of the trigonometric functions, their development in series and methods of computation, the relations between plane and spherical triangles, and the applications of the subject noted above. These last are very complete in the matter of problems, and furnish excellent work for drill purposes.

The general appearance of the book is excellent, the typography clear, and the figures well drawn and distinct.

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*Photographic Mosaics* By E. L. Wilson, 833 Broadway, New York, 1891.  
Price, 50 cents; cloth, \$1.

Photographic mosaics, the oldest of the American photographic annuals, has been enlarged to double its former size, and contains 284 pages of matter of interest and value to photographers, besides 12 full-page process illustrations. Of its 284 pages, 112 are devoted to a retrospect of last year's work, and the remaining 172 pages consist of original communications. I.

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Mr. ROBERT GRIMSHAW, General Editor of the Trades Department of *Funk & Wagnall's Standard Dictionary*, being desirous of making as complete as possible his list of mechanical and industrial terms, requests manufacturers of machinery and tools having important parts not found on those of other makers, or the names of which are not yet in general use, to send the name, definition, and use of each such part to him, at 115 Bible House, New York City.

## GIFTS TO THE LIBRARY OF THE FRANKLIN INSTITUTE.

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- Adams, H. C. Gas Coals in the United States. From the author.  
Agricultural Experiment Station of the University of Wisconsin. Seventh  
annual report. From the station.
- Aid to Astronomical Research. No. 2. From Prof. E. C. Pickering.
- Alabama Agricultural Experiment Station. Bulletin No. 20.  
From the director.
- American Ephemeris and Nautical Almanac for 1893.  
From the superintendent of nautical almanac.
- American Society of Mechanical Engineers. Transactions. Vol. 11.  
From the society.
- Baker, Henry B. Malaria and the Causation of Intermittent Fever.  
From the author.
- Brooklyn Library. Bulletin No. 28. From the library.
- Connecticut Board of Agriculture. Twenty-third annual report of the  
secretary. From the state librarian.
- Connecticut Public Documents. 1890. Vols. 1 and 2.  
From the state librarian.
- Delaware College Agricultural Experiment Station :  
Bulletin No. 4. From Joseph M. Wilson.  
Bulletin No. 10. From the station.
- Engineers' Society of Western Pennsylvania. Proceedings of meetings of  
September and October, 1890. From the secretary.
- Foote, Allen R. Dangers to Firemen from Electric-light Wires in Build-  
ings. From the author.
- Germanischer Lloyd. Internationales Register. 7ter Nachtrag, 1890.  
From L. Westergaard & Co.
- Grand Trunk Railway of Canada. Report for half-year ended June 30, 1890.  
From Sir Joseph Hickson.
- Harvard College Observatory. Annals. Vol. 21. Part 2. Vol. 24. Vol. 30.  
Part 1.
- History of Harvard College Observatory. From the director.
- Illinois State Laboratory of Natural History. Articles 5 to 10.  
From the director.
- International American Conference. Minutes. From the executive officer.
- Manchester Public Free Libraries. Thirty-eighth annual report to the  
council of the city. From the librarian.
- Massachusetts State Agricultural Experiment Station. Analyses of com-  
mercial fertilizers. From the station.
- Massachusetts Institute of Technology. Commemorative address by  
Augustus Lowell, Esq. From the institute.

ANNUAL REPORT OF THE DIRECTOR OF THE DRAWING SCHOOL  
OF THE FRANKLIN INSTITUTE FOR THE SESSIONS 1890-91.

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The year which closes this evening has been the most successful one in the history of the school, both in the number of pupils and in the system pursued in the teaching. In the department of mechanical drawing particularly the course is so well graded and so complete, without containing anything useless or merely time-consuming, that the results have in many cases been surprising. All theories have been made to conform to the best known practice, and the two have been carried along in harmony. This has been possible because the instructors have not merely received the conventional school-training in drawing but have all had practical experience.

The use of the new text books has been of very great service in ensuring systematic methods, and in economizing the time of both pupils and instructors. This has shown so plainly in the mechanical classes that an effort will be made to have one ready for the architectural class before the next term begins.

Architectural drawing, in many respects, is not in so advanced a condition as engineering drawing, because the methods of architects have been selected and adapted to suit the average contractor in the building trades, whose estimates on the cost of work are apt to be increased, as experience has shown, by the perfection of the drawings and the degree of their exactness. This may eventually be overcome when the trades schools, which are being established, have had time to produce their effect.

The free-hand department maintains its high standard, and its product is of a character to be understood and appreciated by the laity, and to speak for itself. Special attention has been paid to pen-and-ink work with very gratifying success.

The instruction throughout the school has been individual, class instruction with black-board demonstrations having proved a failure, owing to the great difference in the qualifications of the pupils. When each individual is treated on his own merits, he can be strengthened where he is weak and is not held back where he is strong.

The re-arrangement of the class-rooms, which was effected at the beginning of the preceding year, for the purpose of accommodating the entire school on two evenings each week, has again been insufficient this year, and the school has had to be divided into two sets on different evenings. As is the case with all the other departments of the Institute, the drawing school is cramped for want of room and accommodations.

The success of the school is largely due to the ability of the instructors, Messrs. Clement Remington, George W. Irons, George S. Cullen, John F. Rowland, Lucien E. Picolet and H. Allen Higgins.

The following pupils are entitled to Honorable Mention:

*In the Junior Classes.*

Joseph H. Moore,  
August Mylander,

William T. Westbrook, Jr.,  
Benjamin H. Barton,

Herman Klinefelter,  
Frank Hopkinson,

Charles H. E. Price,  
Frederick R. Baker.

*In the Intermediate Classes.*

L. C. Dilks,  
Walter Hensel,  
Frank J. Crowne,  
William E. Arnold,  
William Weinrich,  
C. Q. Hewitt,  
Thomas Worn,  
William S. Cooper,

John Rigling,  
David W. Perry,  
Henry R. Johnson,  
A. S. Mackenzie,  
George T. Coleman,  
Edgar R. Eavenson,  
Julius Block,  
J. Lyndall Hughes.

*In the Senior Mechanical Classes.*

Dinwiddie J. Luckett,  
William L. Miggett,  
William Hayes,  
Eustace Rimmer,  
John Bayne,

W. H. Sheahan,  
R. H. Trimble,  
M. A. Sykes,  
James Mawson,  
Charles Kolb.

*In the Architectural Class.*

A. Albertson,  
Frank L. Borie,  
Benjamin G. Barnett,

Henry Kerr,  
Richard T. Given,  
Wesley L. Blithe.

*In the Free-Hand Class.*

Robert F. Schleicher,  
Walter L. Allen,  
G. A. Davison,

August Schimpf,  
J. G. Sullivan,  
Thomas Winterbottom.

The following pupils are awarded scholarships from the B. H. Bartol Fund, entitling them to tickets for the next term, beginning September 22, 1891 :

W. H. Sheahan,  
L. C. Dilks,  
Walter Hensel,  
William T. Westbrook, Jr.,  
Robert F. Schleicher,

William L. Miggett,  
John Rigling,  
Joseph H. Moore,  
August Mylander,  
August Schimpf.

The following pupils, having attended four terms and completed satisfactory a full course, are awarded certificates to that effect :

Benjamin G. Barnett,  
Wesley L. Blithe,  
Philip Bradley,  
James J. Burke,  
Frank A. Butz,  
Joseph C. Douglass,  
William H. George,

John Bayne,  
Frank L. Borie,  
Charles Burk,  
Arthur H. Burling,  
Michael Carlin,  
Charles Friend,  
Charles L. Henry,



H. Allen Higgins,  
 Charles B. Jarden,  
 Charles Kolb,  
 W. A. Leavitt, Jr.,  
 James Mawson,  
 John A. Mooney,  
 Eustace Rimmer,  
 Isabella Scott,  
 Claudius M. Tice,

Walter L. Holbrook,  
 Frederick Koeberle,  
 Philip J. Lauber,  
 Dinwiddie J. Luckett,  
 James Miles, Jr.,  
 D. O'Brien,  
 Charles J. Rooke,  
 Scott Wolff.

The next term of the school will begin September 22, 1891, and everything possible will be done to increase the efficiency and high standing of this department of the Franklin Institute.

WILLIAM H. THORNE,

*Director.*

*Philadelphia, April 23, 1891.*

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## Franklin Institute.

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[*Proceedings of the stated meeting, held Wednesday, April 15, 1891.*]

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HALL OF THE FRANKLIN INSTITUTE,  
 PHILADELPHIA, April 15, 1891.

JOS. M. WILSON, President, in the chair.

Present, 132 members and ten visitors.

The secretary presented on behalf of the Committee on Science and the Arts, the report of the committee on the Anderson system of water purification. The report was discussed by Mr. Easton Devonshire and others.

Mr. Geo. D. Burton, of Boston, Mass., read a paper on a new method of forging, rolling and shaping metals with the aid of electric heat, the system being of his invention. The paper was discussed by Mr. Wiegand, the secretary and the author. Mr. Burton illustrated his paper by an exhibition of a number of specimens of metallic articles formed by his process.

The secretary's report embraced a description of the plan for utilizing the water-power of the Niagara Falls on a large scale; also, a description with illustrations of the system and appliances successfully employed, in Russia for consuming oil as fuel on the locomotives in service on a number of railways in that country. This subject was discussed by Messrs. John T. Haskins, S. Lloyd Wiegand, W. D. Lockwood, James Christie and others.

Mr. W. N. Jennings exhibited and commented on a series of photographic views of local interest, which attracted much attention.

The meeting then proceeded to an election to fill a vacancy in the Committee on Science and the Arts, caused by the resignation of Mr. Henry Pemberton, Jr.; Mr. Philip H. Fowler was chosen to fill the unexpired term of the retiring member.

Adjourned.

WM. H. WAHL, *Secretary.*

# JOURNAL

OF THE

# FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA.

FOR THE PROMOTION OF THE MECHANIC ARTS.

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VOL. CXXXI.

JUNE, 1891.

No. 6

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THE Franklin Institute is not responsible for the statements and opinions advanced by contributors to the *Journal*.

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## THE PROGRESS OF CHEMICAL THEORY: ITS HELPS AND HINDRANCES.

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BY DR. PERSIFOR FRAZER, Prof. of Chemistry.

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[*Introduction to the chemical course. A lecture delivered before the Franklin Institute, November 21, 1890.*]

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[*Concluded from p. 334.*]

For example, in the phosphorus and arsenic salts only those which had like composition and like number of equivalents of water were isomorphic. Berzelius used both of these as guides but showed a preference for the dictum of his former scholar. In his later table of atomic weights of 1826, Berzelius corrected all but the alkali metals which he still considered united to oxygen in the proportion of 1 to 1.

It will be recalled that Sir Humphry Davy and his school had pronounced chemical inseparable from electrical activity.

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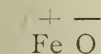
He had shown that when a compound was decomposed by the electric current and the separate constituents collected at the opposite poles (as for example sulphur and copper) these substances when rubbed or electrically excited showed opposite kinds of electricity: the substance at the negative pole showing positive, and that at the positive pole negative electricity. If the separate constituents (such again as sulphur and copper) were heated, the heat increased their electrical tension up to the point where they combined, and then all electrical activity ceased, the opposite kinds neutralizing each other.

In conducting the current into the compound its constituents received again electric polarity and separated to the respective poles which attracted them.

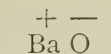
Berzelius first stated his electro-chemical theory in 1812. His fundamental conception was that the atoms of bodies are electric and therefore have at least two poles which generally are different in strength, and following this difference the elements which these atoms compose are electro-positive or electro-negative. This predominance of one kind of electricity extended to compounds, although necessarily more feeble in these. Compounds, according to this view, are the results of the attraction of the unlike poles of the atoms; and if in the compound there is a preponderance of one kind of electricity, this is because the atoms having this kind were more strongly polar than those having the other. Oxygen as the most electro-negative of all bodies was his criterion of the characters of the elements with which it combined. If the compound containing the least oxygen was basic then the body combining with oxygen was electro-positive. If the oxide were an acid then the element was electro-negative.

On this plan he arranged a table of elements in the order of their polarity. Of course, it followed that any body in this table was electro-positive to all the elements on one side of it and electro-negative to all those on the other.

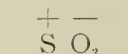
His dualistic scheme followed as a matter of necessity, the two parts (whether two elements or acid and base) having opposite polarities. Thus:



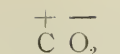
Ferrous oxide.



Barium oxide



Sulphuric acid.

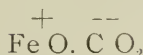


Carbonic acid.

or for salts.

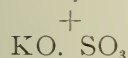


Baryta Sulphate.

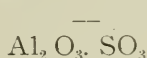


Ferrous oxide Carbonate.

and in double salts.



Potash Sulphate.



Alumina Sulphate.

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 Potasso aluminum Sulphate.

Berzelius carried his dualism and his electro-chemical theory into organic chemistry. In 1813 he had discovered a simple relation between the elements composing the organic acid and the oxygen of the base, and had proved the combination in multiple proportions in organic chemistry, and it was he who recommended the study of organic bodies in their combinations with inorganic.

He supposed organic, like inorganic, bodies to be binary in constitution, but with compound radicles instead of simple elements. This was Lavoisier's idea. Gay-Lussac had characterized alcohol as consisting of ethylene and water; sugar as carbon and water. Döbereiner called oxalic acid a compound of carbonic acid and carbonic oxide. Berzelius opposed this conception as inconsistent with his electro-chemical theory. He could not acknowledge the existence of oxygen radicles. Yet the probability of such radicles was rendered strong by the discovery of organic bodies containing the same numbers of the same elements, yet exhibiting very different properties. This could only be explained by supposing that the proximate constituents were different.

Wöhler, Liebig and Berzelius, after much hesitation, accepted the fact above referred to and the latter designated the phenomenon as one of isomerism. Liebig and Wöhler had shown that a constituent of the oil of bitter almonds remained unchanged throughout a number of reactions, and

this compound, of which the composition was  $C_{14}H_{10}O_2$ , they called benzoyl. (It is now called dibenzoyl or benzil.)

At first Berzelius was disposed to accept this as an organic compound radicle,\* but reflecting that it must play the electro-positive rôle, although containing oxygen, he finally rejected this hypothesis which he deemed inconsistent with his electro-chemical and dualistic theory; and he was led to the assumption of arbitrary radicles containing no oxygen, of which the formulas, when written together, completely obscured the intimate relationships which existed between classes of salts. This was Berzelius' first fault, not so much due to his vanity, as to the feeling which was well founded that the scheme he had with such infinite pains established was right; that it was being destroyed on theoretical grounds which, although he could not then satisfactorily answer them, with the instinct of a great genius, he felt to be wrong. But his attempts to evade the conclusions only led him into self-contradictions which, when exposed, produced the same effect upon the chemical mind that the reported insolvency of a great banking house produces on the financial world. The failure of a Berzelius shut up the current coin of theorizing the world over.

Berzelius believed that the radicles were unchangeable. Liebig and Dumas were not convinced of their entire unchangeability. The two sets of views separated more and more. Liebig finally defined a radicle as one which must be—(1) an unchangeable unit in a number of compounds. (2) It must be replaceable in these by simple bodies. (3) It must allow the simple bodies with which it is combined to be replaced by others, to form with it new compounds. This was the old radicle theory.

In 1827, J. B. Dumas (1800–1884) commenced a series of researches on the vapor densities of many substances which are solid or liquid at ordinary temperatures, and showed that if Gay-Lussac's law of the identity of atom-weight with gas-volume-weight be true, then some of Berzelius' atom-weights were double and some were half what they should be. This cast doubt on the truth of the law as well as on

\* Spelled also *radicol*—*radicle* is used by Watts.



Berzelius' work, but the latter held fast to his numbers (which were right), and simply confined his employment of the volume theory to permanent gases. The effect upon the minds of chemists, however, was disastrous; for even Gay-Lussac, Liebig and others, despaired of getting any information as to the atoms, and fell back on the equivalents which were shortly afterwards more precisely defined by Faraday as the contemporaneous quantities of the various constituents of compounds which an electric current of given intensity would disengage. This he called the law of constant electrolytic action.

Under the unfortunate fear of having been misled, all attempts at theory were suspended. Gmelin in the colossal work which bears his name went back to the apparent weights of combination of Lavoisier's time, and the soul was taken out of the science. But Dumas dealt Berzelius a heavier blow still in the field of organic chemistry in his substitution theory, which he called "metalepsie," of which the two propositions were:

(1) If a hydrogen compound is subjected to the action of chlorine, iodine or bromine, it takes for every atom of hydrogen lost an atom of one of these elements to replace it.

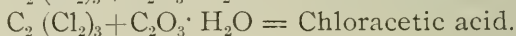
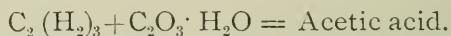
(2) If the body contain water, it loses this without replacement.

Auguste Laurent (1807-1853) went further and asserted that the congeries of atoms called radicles which had suffered this substitution of H by Cl, or the loss of water remained practically the same in properties. This was called the nucleal theory which was short-lived, but before it had quite disappeared from the field Dumas came forward with his discovery of chloracetic acid, and declared himself for Laurent.

He maintained that the newly-formed body in such substitutions must resemble the old and he classed such original and substituted products together as derivable from the same type. This is called the old-type theory to distinguish it from the new-type theory subsequently set up by Laurent and Gerhard. Dumas showed that his trichloracetic acid in spite of the substitution of six atoms of chlorine for

six atoms of hydrogen was similar in its behavior and characteristics. He stated that "In organic chemistry there are certain types which remain constant even when one substitutes an equal volume of Cl, I, and Br for their H." Acetic and trichloroacetic acid, aldehyde and chloral, marsh gas and chloroform, belong to the same chemical type. He went so far as to assert that all bodies derived from each other by equivalent substitutions belonged to the same "mechanical type." This was followed by the following declaration of war against Berzelius' dualistic theory. "Every chemical compound forms a complete whole, and does not consist of two parts. Its chemical character depends principally upon the number and arrangement of its atoms, and secondarily upon their chemical nature."

Berzelius was obliged to account for the facts discovered and to conform his explanation to the new discovery of Melsens in 1842, that chloroacetic acid was reduced to acetic acid by potassium amalgam. He finally decided to do this by supposing that



In other words that acetic acid consisted of a radicle ( $\text{C}_2\text{H}_6$ ), and trichloroacetic acid of the corresponding radicle ( $\text{C}_2\text{Cl}_6$ ), each respectively paired with oxalic acid ( $\text{C}_2\text{O}_3 \cdot \text{H}_2\text{O}$ ).

But in doing this he gave up the very principle that he had been contending for, viz: that the radicles were unalterable: since it was evident that in the first two symbols of the formulas a radicle  $\text{C}_2\text{H}_6$  had been changed by substitution into  $\text{C}_2\text{Cl}_6$ .

This admission weakened the faith of his most admiring followers and threw the blight of timidity over the speculations of chemists for thirty years. And yet withal, Berzelius was right in almost every important generalization which he made, and right (within the resources of his time) in every constant he established. Chemistry without his contributions would yet be a mere scaffolding.

The history of this controversy, the most important which has taken place since the commencement of the

modern science, is useful as an illustration of another kind of hindrance to the progress of intelligent theory. It is a too blind devotion to one man, and despair of attaining the object which he strove for if he has failed. It is true that if ever hero worship were justifiable that of Berzelius was, but to sweep aside the whole fabric of his labors, because part of his theory was proved untenable, was the veriest cowardice and injustice to him.

How many a chemist in the last forty-five years has struggled through a jungle thick planted with the pretty crochets of mediocre men, wondering what all this jargon meant and whither it was tending; deprived of the vivifying influence of a high and noble thought, and condemned to delve and grub without reasoning, because one of the greatest geniuses of original research the world has ever seen was worsted in an encounter on a small part of his field, and a part to which he had not devoted the best of his thought!

Among the most innocent looking hindrances to the development of sound chemical theory must be reckoned an hypothesis, which was given anonymously to the world in 1815, and which was later referred to its true author, Dr. Prout. This was the supposition that the atomic weights of all elements were even multiples of that of hydrogen.\*

No argument was offered to show why this should be true, but it had the specious and enticing appearance which captivates that sense of order which is a human instinct.

Prout did not hesitate to add the obvious conclusion that hydrogen was the original material, or as one of the greatest of living chemists has put it, the prototype.

By the usual rule which exists in such cases Prout should not have had credit for anything more than a pretty idea which besides was only a modification of a similar thought of Thomson the collaborator of Dalton, who believed

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\* It should be stated that Profs. Crookes, F. W. Clarke and other chemists of the first rank still show a leaning towards the acceptance of something like Prout's "law."

that the atomic weights of the elements were multiples of that of oxygen. Prout was more nearly right, of course, because the weight of hydrogen is only about one-sixteenth as great as that of oxygen, and is a more general divisor. But in spite of all, this pretty thought strongly influenced many admirable chemists, even those like Dumas and Stas and Marchand and Erdmann, whose labors in accurately determining atomic weights did more than all else to prove the baselessness of Prout's assumption.

Even to-day the tendency is manifested to get back to the even numbers which Leopold Gmelin adopted in his great work, in entire disregard of the accurate results of Berzelius. It must be added that some are prevented from falling into this snare through the hope that the real protyle will be some day discovered, and that being lighter than hydrogen it will provide a table of atomic weights less disfigured by fractions than the present one.

The moral of this theory is that healthy scientific work must not be influenced by premature attempts to put a finish on it and make it appear symmetrical.

The discovery of an element lighter than hydrogen and possessing properties which, as it will be seen, we can to a certain degree predict, might render very desirable those ugly decimal affixes to the present atomic weights in order to show that experiment supported theory. On the whole Prout's "law" as it is called is a hindrance and quite an annoying one.

The downfall of dualism and the establishment of unitism in compounds was followed by a storm of conflicting notions of chemical union into all of which it is not necessary to enter.

To Laurent and Gerhardt is due the praise for unifying in the new theory of types, the opinions which had been previously thought to be diametrically opposite. It has been said that the uncertainty which prevailed upon the overthrow of the dualistic theory of Berzelius caused most chemists to go back to the old equivalent weights  $O = 8$ , but Gerhardt opposed this weakness and strongly maintained the correctness of the numbers of Berzelius, merely correcting

his early atomic weights for the alkaline metals in conformity with the then acquired knowledge of the existence of two atoms of the metal in the alkalis. He showed that the quantities of  $\text{H}_2\text{O}$ ,  $\text{CO}_2$  etc., separated in the reactions of organic compounds did not represent one equivalent each, but two. His aim was to reduce all formulas to one criterion which in the case of gaseous substances was two volumes. His view was that, when two bodies react, one constituent of one (H), uniting with one constituent of the other (O), leaves two residues which unite. Hofmann discovered the substitution products of ammonia and thus established the ammonia type. Williamson established the water type and referred alcohols, ethers, acids, bases and salts to this type. Gerhardt recognized the hydrogen type, the water type, and the ammonia type, but considered the compounds under them units and not couples.\* Laurent explained the atom as the least part of matter that can take part in chemical combination; the molecule as the least part which can exist alone, and the equivalent of elements as those quantities which would perform the same amount of chemical work.

Thus the new-type theory was a compromise skilfully constructed to save the Berzelians from chagrin while embodying all of value in their system. It took the idea of type from Dumas, that of radicle from Berzelius, but with the difference that it did not presuppose the groups of atoms called radicles to be necessarily capable of existing alone. Gerhardt admitted that the elements substituted in radicles did change the character of the compound.

Kekulé, in the year of Gerhardt's death (1857), added the conception of mixed types.

It is unnecessary to pursue this branch of the theory farther. An entirely new discovery appeared which was a

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\* Dr. Wolcott Gibbs, in 1858, after referring to his attribution of the theory of water types to Gerhardt and Williamson, says: "I have not done justice to T. Sterry Hunt, to whom is exclusively due the credit of having first applied the theory to the so-called oxygen acids and the anhydrides; and in whose earlier papers may be found the germs of most of the ideas on classification usually attributed to Gerhardt and his disciples."—*Chem. and Geol. Essays*, p. 468.



welcome light and enabled many of the obstacles in the way of progress to be seen and surmounted.

Frankland, after an investigation of the alcohol radicles, first announced his views of valence in 1853.

Kolbe, in 1855, accepted his theory and declared that the fatty acids, if imagined free of water, were derivatives of  $\text{CO}_2$  or  $\text{C}_2\text{O}_4$  in which one atom of oxygen is replaced by  $\text{C}_2\text{H}_3$ .

Frankland's discovery was really a corollary of multiple proportions, and had been vaguely foreshadowed by Wöhler, who said that one atom of antimony was equal to three of hydrogen.\*

It is simply, as Hofmann expresses it, chemical value in exchange. Some elements, of which H is a type, can only combine one atom to one atom; some, like oxygen, can hold two atoms of the first-named kind in union. Some, like nitrogen, can hold three, and some, like carbon, four. Kekulé, in 1858, determined this valence of carbon, added another to the list of types which led to the establishment of the ring structure of some organic compounds.

Elenmeyer found that every element had a highest valence but might not use all of its combining powers. Wurts and Naquet believed the valence changeable, Kekulé believed it fixed, and that only compounds using all the valences of every constituent were real chemical compounds. It appears to be the general opinion nowadays that the valence of elements is not only variable but that it does not even vary as was supposed by the suppression of two affinities at once (which might be explained as the loss or suppression of one whole polarity); but that an element may be now an artiad and now a perissad, *i. e.*, even or odd in the number of its bonds.

The subject is not well understood, but it presents a good opportunity for acting on the lesson taught by Prout's "law," not to be captivated by a beautiful idea, but to stick closely to facts.

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\* Von Meyer's admirable and impartial work on the history of chemistry (Leipzig 1889), a most valuable contribution to the science and a fitting supplement to Kopp's, has been largely drawn upon.

It would be impossible at this time to follow all the subsequent contributions to the theory of chemistry, but the most superficial sketch of the subject would be incomplete without studying an extraordinary discovery of which the advent had been preparing long before its announcement by Lothar Meyer and D. Mendelejeff. The latter pointed out that if the atomic weights of the light elements (or those with atomic weights from one to thirty-six) be arranged in two lines of seven each, a natural grouping is effected thus:

Li-7. Be-9.4. B-11. C-12. N-14. O-16. F-19  
Na-23. Mg-24. Al-27.3. Si-28. P-31. S-32. Cl-35.5.

In these two lines two periods are passed over. From left to right in each, with the increase of atomic weight is a change from the most electro-positive to the most electro-negative, while the elements in the centre are nearly neutral. Again with the beginning of the second period, elements of the same kind come to stand under one another. The same might be shown to be true in their behavior in forming compounds with oxygen. Again the metals are on the left and the non-metals on the right. The specific gravities increase, and the atom-volumes (or the atomic weights divided by the specific gravities), and all other physical and chemical properties which have been examined, change by regular gradations in vertical or horizontal lines. Without pursuing this subject into its curious details it is apparent that there has been some law exemplified in the production of these elements; a condition of things which is cyclical in its action or, as it is called, periodic. From the data obtained it was possible to construct a table which while containing all the known elements, contained gaps in certain places where (according to the analogy of the periods) elements ought to have been but none had been discovered. From the position of these gaps it was even possible to prophesy from the analogy of elements in similar positions of other periods, that if discovered the new elements filling these gaps would have properties intermediate between those occupying places before and after it, in regard to fusing point, solubility, specific heat, positive or negative polarity, etc.

After the announcement of this periodic law, chemists were impatient for a verification of its accuracy, and in the discovery of the newest metals, gallium and germanium, those predictions were found to have been well founded.

As no better test of a theory can be had than its use as a basis of prediction, the periodic law may be said to have been proved, and to be evidence of a profound and intimate connection between the elements.

What the connection is has not been certainly ascertained, but a bold and beautiful hypothesis was enunciated in 1886, by one of the greatest of the master minds of our age, in his presidential address before the Chemical Section of the British Association for the Advancement of Science, William Crookes.\*

A skeleton of the views in this paper is as follows :

Norman Lockyer said that a terrestrial element is an exceedingly complex thing, broken up into simpler things at the temperature of the sun.

Sir Benjamin Brodie in 1867 said, "We may conceive that in a remote time or in remote space there existed formerly or may exist now certain simpler forms of matter than we find on the surface of our globe."

Professor Stokes, in referring to a line in the spectrum of the nebulae remarks: "It may possibly indicate some form of matter more elementary than any we know on earth."

Crookes holds that the unequal distribution of elements in the earth's crust is evidence in favor of their composite nature.

The probability of such rare metals as yttrium, ytterbium, etc., having been brought together by chance in a few uncommon minerals and in a few localities, is very small. They would rather seem to have been formed severally from some common material placed in conditions in each case nearly identical; and the existence of other groups

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\* See address of William Crookes, President of the Chemical Section Brit. Ass'n for the Adv. of Science, at the Birmingham meeting, 1886. (R'pt B. A. A. S., 1886, p. 558.)

of metals, as nickel, cobalt, and the two platinum groups furnishes additional ground for this supposition. Another argument in favor of their composite nature is that of the organic compound radicles.

Dr. Carnelly at the previous meeting (1885) had shown that on the supposition of two chemical elements; one with an atomic weight of twelve and the other of two—all the features of periodicity in Mendeleeff's series could be produced by their combination, and every well-known element reproduced except hydrogen.

Dr. E. J. Mills considered the elements we now have as the result of successive polymerizations.

All these observations Crookes has thrown into a diagram by which is represented the hypothetical condensation of the original "fire mist" out of which the universe was derived and which contained, not matter but the potentiality of matter. In this condensation due to the gradual lowering of the temperature, another force than that of heat was at work—a force which he supposed not very different from electricity. As the temperature was lowered the elements congealed one by one out of the protyle in the form of atoms, of which the weight depended upon the temperature at the time of this formation, and the properties due to the electrical phase at the time of their birth. But the potential energy of the atom was greatest in the first that were thus condensed and more sluggish in the last when the temperature was lowest and the electrical force least.

We must be warned by the very beauty of this conception that it is but the effort of a superior mind to materialize for us the successive phenomena which it sees in imagination, and not a register of observed phenomena or a proven genesis. Yet it is impossible to conceive of the regularity and accuracy with which the labor of a century is fitted into that scheme without feeling that it contains more than a figment of the fancy.

At least nothing greater and nobler has been attempted in our science since its inception, and whether it be finally assigned its place among the great theories of the world or not, it will, undeniably, through its broadening of our

views and its enlarging of our conception, have been a help to the progress of chemical theory.

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In reference to the supposed steam engine prediction, mentioned at the beginning of this lecture, Mr. W. P. Tatham calls my attention to the fact that the actual observation misquoted by the *N. Y. Herald* for sensational or other satisfactory journalistic reasons, and repeated ever since (in spite of countless corrections), after the manner of the average erroneous newspaper paragraph, was made by Dr. Dionysius Lardner and referred to the steamer of that epoch, which, according to his calculation, could only carry coals for a journey of 2,000 miles with due allowance for accident and delay. That he never entertained such an opinion as that above referred to is evident from the following language: "We are even now upon the brink of such improvements as will probably so extend the powers of the steam engine as to render it available as the means of connecting the most distant parts of the earth."

The steam engine familiarly explained and illustrated, etc., by the Rev. Dionysius Lardner, LL.D., F.R.S., pp. 241-242, etc., etc. E. L. Carey and A. Hart, Philadelphia, 1841.

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## POSSIBILITIES OF APPLIED SCIENCE.

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BY OBERLIN SMITH.

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[*A lecture delivered before the Franklin Institute, February 16, 1891.*]

The lecturer was introduced by the Secretary of the Institute, and spoke as follows:

LADIES AND GENTLEMEN :—When the flattering request that I should address this learned Association of the devotees of Science and Art was tendered me by your Secretary, I hastily chose the theme, "Possibilities of Applied Science," as one well fitted to an institution which is, let us hope, a congenial haunt of the spirit of the immortal Franklin. It has occurred to me later, when asked whether I needed a lantern and screen, or other illustrative apparatus, that perhaps the absence of these accessories will cause disappointment to my audience—inasmuch as I have used them here to some extent upon a former occasion, and as many of my fellow-lecturers have thus made for you brilliant expositions of science, after a manner that no speaker can do



by mere words—however eloquent. Possibly the word “Future” should have been prefixed to my title, as some of my hearers may have expected to see illustrated this evening things that, though strange and new, have already been accomplished. Had I been dealing with the science of the present, this expectation might have been fulfilled. As, however, this ground was so well covered by my colleagues in this course, I felt relegated to the *future*, or the *past*. When thinking over the latter theme shall I confess to a hope lingering in my mind that some of our ever-active oriental explorers would soon unearth certain photographs and detailed blue-prints of their work, left in trust to the Franklin Institute by Tubal Cain, Hero of Alexandria and the spear-makers of Cæsar’s Rome. From these you should have had lantern slides, but, alas, I found that I had heard in vain Wendell Phillips upon the “Lost Arts,” and that I must cry, with Milton,

“The past, who can recall, or done, undo?  
Not God omnipotent, nor Fate.”

Turning to the future for my subject, I was not yet out of my dilemma—finding that even our most progressive amateur “camerists” had found illustrations of this theme to be among the *impossibilities* of science. Mental pictures, however, we may limn, for

“Time, as he courses onwards, still unrolls  
The volume of concealment. In the future,  
As in the optician’s glassy cylinder,  
The undistinguishable blots and colors  
Of the dim past collect and shape themselves,  
Upstarting in their own completed image,  
To scare or to reward.”

Could Franklin be with us to-day and see the marvellous developments that have taken place in all branches of science as applied to the arts, in the short century since he reigned as one of the kings among inventors and physicists, can we think he would be skeptical regarding the ultimate fulfilment of any mental prophecies which your lecturer’s flights of fancy may lead us mutually into this evening? Would he not excuse us from the charge of being unduly

speculative and visionary, were he to measure the giant's strides, impressed indelibly in footsteps of light, upon the last decade of our history, even deeper than upon its predecessors?

Within this brief period, to say nothing of the many and brilliant inventions which mark the few previous decades of a half a century back, we have seen in our midst the absolute creation of six great commercial industries founded upon electricity alone. I refer to telephony, electric lighting, electric railroading, electric power-transmission, electric welding and electric reduction of metals—notably aluminum. Besides these well-established applications to the purposes of life of that mysterious steed which Franklin loved to harness, any one of which would have marked an era in the earlier ages of our civilization, we are daily hearing of new burdens that this fiery charger is being taught to carry. Among these a few prominent ones that occur to me are electric tanning, electric ore-drilling, electric hoisting, electric warming, electric cooking, electric navigation and tube-making by electro-deposition. With all the hum of electrical industries so constantly about us, it seems difficult to realize that only a score of years ago our practical commercial use of electricity, on a large scale, was limited to telegraphy, electro-plating, bell-ringing, and perhaps the killing of a few farmers, whom the "lightning-rodders," as the genial Stockton calls them, had persuaded to put their rods almost entirely above ground instead of partly below.

Knowing of these marvels in the near past, and of all the other wonders of our century, in all departments of engineering science, and knowing that many of them were, a few years before their appearance, regarded as impossibilities, even by men of science, I do not feel that I am over-visionary in suggesting the things to be mentioned later on as within the realms of possibility. Some of them are more than this; they are probabilities, while not a few may be almost classed as certainties—as far as we can, with our present knowledge, predict their logical future development from their past history.

Who, for instance, knowing that we in Philadelphia now talk distinctly to business associates in Boston or Buffalo, in an ordinary tone of voice, the individuality of which is clearly recognizable, and knowing that even the wisest among us would have declared the thing absurd fifteen years ago, is willing to declare that it is impossible that we shall sometime see each other a thousand miles apart? No one, I think, who has a keen eye for the brilliant analogies which science is ever holding up before us, as object lessons, with which to teach us of her lore.

When, a few sentences back, I spoke of choosing in haste this theme, the expression was used in reference to the fact that it had been found more unmanageable than I at first supposed, on account of certain difficulties in classification. In trying to group under appropriate heads the sub-subjects we wish to examine together this evening, an obvious method seemed to be to treat them as manifestations of science applied to the various arts by which man supplies his wants, and to take these somewhat in their natural order of importance, in the development of civilization out of barbarism. The chief of such wants and desires are: food, clothing, shelter, warmth, light, trade, travel, learning, art and health. This classification is convenient, rather than strictly logical, as some of the terms partly include some of the others.

A more scientific framework for my discourse, but one which, for reasons to be mentioned, seemed likely to result in some confusion, would consist in a system of grouping by the sciences themselves: as, Agriculture, Architecture, Chemistry, Engineering, etc. In trying to analyze the latter term, and fix upon appropriate adjectives for the numerous departments of modern engineering science, I was more than ever impressed with the inadequacy of our nomenclature in the matter of such definitions. In a comprehensive sense this science includes all the others, as applied to the useful arts, and, treating the subject with some degree of logic, we have Engineering, agricultural, textile, architectural, calorific, illuminant, topographical, hydraulic, mining, metallurgical, marine, vehicular, acoustic, aeronautical,

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chemical, educational, surgical, diversional, military, etc. The most used adjective of all, "civil" does not appear in this list, because, in its true sense, it includes all the others—except the last named. So also the terms "mechanical" and "electrical" engineer are difficult to define exactly because, according to modern methods, their work enters largely into that of all the others mentioned, and includes part of the military field. Their functions are also somewhat interlocked with each other, so that they are all the time becoming more and more interdependent. From the somewhat awkward list given, which, by the way, is far from complete, it will be seen that the idea involved cannot be carried out without the coinage of certain words which are, to say the least, rather unconventional.

Upon the whole, it seems that the deeper one tries to look into the work of a systematic classification of applied science, the more befogged becomes one's vision. The truth of the entire matter is that we sorely need some congress of dictionary-makers—not only for the fixing with exactitude a meaning for many general scientific terms, but for the establishing of definitions that are definite, for thousands of words pertaining to the details of the technical arts. In ending this digression, it might be fitting to approach the subject proper of my discourse by suggesting that among the brilliant possibilities of the future will be some feat of lexicographical engineering, so to speak, that will apply the rigid logic of our modern chemical nomenclature (which has been the means of so much advancing that science) to the various other branches of technics. Let us all hope for the day when, in the world of science, no two words will mean the same thing, and no two things will be described by the same word.

Following the classification of our subject first proposed, we shall find perhaps in the realm of *food-making* less opportunity for important changes than in any other field of human industry—unless, indeed, we possibly may sometime reach what would be the most radical change in all human development, namely, the artificial production of food from inorganic substances, or even from woody fibre. Such pro-



duction would, of course, tend to utterly revolutionize our agriculture, our fisheries, our stock-raising and game-preservation, together with all the methods of commerce dependent thereupon. A change of this kind seems extremely unlikely to happen for many hundreds or thousands of years to come; and yet, in view of recent developments in chemical synthesis, which prove that many heretofore-considered organic substances can be built up from elements derived in the laboratory from inorganic matter, it will not do for us to rate it as an impossibility. In the making of our present foods we have not, perhaps, very much to learn, because such making is the oldest industry in the world. In the art of cooking them, however, we are still progressing, as we have been since some period following Adam's little pomological experience, wherein the apples received among his wife's morning marketing were probably not baked ones. We shall, no doubt, learn from science many minor improvements in the construction, preservation and distribution of prepared foods of various kinds, all of which will promote their purity and cheapness. In household economy, we shall probably develop the greatest alleviations of domestic drudgery by the use of electricity to do the necessary heating—and perhaps the cooking also. In this, as in other household requirements, to be mentioned later on, the ideal substitute for the bringing in of coal and ice, and the taking out of ashes and drip-water, would be a wire connection with some distant source of electric energy. The mere touching of a switch to exactly regulate the heat necessary for roasting, broiling, baking, boiling, washing and ironing, as for power, warming and lighting, would, indeed, be a household blessing.

In regard to the making of *clothing* (of cloth and other textile materials), we can say, as with food, that the art is so old that there seems scarcely room for more great inventions. There is, however, room for many minor improvements, in methods of manufacture as well as in the fabrics themselves. Undoubtedly, some better way will be contrived to fit garments to the human body than the crude methods now in use of cutting up whole pieces of cloth, at



the expense of considerable waste, into irregular shaped sections which must again be sewed together. Very probably the future may develop automatic methods of weaving or knitting fabrics to the exact shape required, so that they will be entirely seamless. An instance of this is already seen in the seamless stockings, which are knitted by some of the recently improved knitting machinery, and there seems to be no good scientific reason why the same principle should not be carried out to cover the entire field in question. We can easily imagine as practicable some mechanical method of measuring the individual so that the machine would automatically produce a correct fit to order. An analogy for this is seen in the process used by hatters for taking the contour of one's head—only in one plane, however, rather than the many necessary for fitting a coat.

Another possible departure from present methods might consist in some system of chemical or sedimentary deposition, to take the place of the present methods of spinning, weaving and piecing together. The wonders of mechanical chemistry, if we may so term it, are already so great, that we may expect almost anything in the future.

Another line marking a radical change in costume may possibly extend in the direction of fabrics composed partly of diminutive air-cells, so that they can be inflated more or less, and thus a given garment be made suitable for zero-cold or summer's day. This would be something upon the same principle already practised by the birds, who fluff up their feathers as the temperature grows colder. Whether such garments might be made available for life-preservers at sea, by still further inflating them, is a question that must be relegated to their future inventors.

In the matter of *shelter*, or home-building, as we may term it, the race has long had ample experience, and we cannot, of course, expect radical changes in the general principles underlying the art of building. We may, however, and probably shall, develop important improvements in regard to materials of construction and methods of fastening them together. Already we are approaching a "steel

age" in regard to the building art. Whether some still more desirable metal will in time supersede it, is, of course, a question, but it would seem not unlikely that such a metal might be found in some alloy of aluminum—provided it should ever become cheap enough.

Just here it may be well to say a word in reference to the enormous crop of nonsense which has recently been, and is still being, printed in our newspapers in regard to the wonderful capabilities of aluminum as a material for buildings, bridges, ships and other structures where great strength, combined with lightness, is desirable. It is true that this noble and beautiful metal, which has so recently become among us a commercial entity, is only about one-third as heavy as iron, and one-eighth as heavy as gold, weighing only about three times as much as hard wood. Unfortunately, however, it is not much more than one-third as strong as iron or mild steel; and consequently, there is very little gain in using it for structures of the kind mentioned, except for its beauty and non-corrodibility. This is for the reason that, to secure a given strength in a loaded beam, cable or other structure, we must use about three times the bulk that we would in the case of iron or steel. All these facts, however, do not prove that aluminum is not a large component of some coming metal suitable for the purposes in question, and many others requiring great strength and lightness, which lightness, by the way, is not so essential in buildings as it is in bridges, ships and railway cars. A few months since, this statement would not have been so pertinent, as no alloy of aluminum was generally known which was very much stronger than the metal itself, unless made largely of iron, copper or some other heavy metal. Within a few weeks, however, information has been made public regarding certain experiments by the French Government which show the valuable result of an alloy of aluminum with only six per cent. of copper. This is more than double the strength of aluminum alone, while its weight is only about six per cent. greater than the weight of that metal. Thus, at one bound is shown the possibility of a revolution in the design of many structures by adapting them to so valuable a material

as the alloy mentioned. These experiments are, however, probably but the first step toward something still better, which we may expect of the metallurgists of the future. With a material very much lighter, in proportion to its strength, than our present iron and steel, we may certainly expect radical changes in various structures, notably roofs, bridges, and the like, whose span can be much increased when some of the limitations incident to present materials are removed.

With regard to the future cheapening of aluminum, it is difficult to predict. The fact, however, remains that its ores exist all about us in probably greater quantities than those of any other material, and that already its cost has been reduced to one-tenth of what it was less than a dozen years ago. Reasoning by analogy, there seems no good cause why it should not soon be brought down to figures which will compete with copper and brass—and perhaps eventually with steel itself. At all events, there are many cases in which we could afford to pay a much higher price than now for the materials of a structure, for the sake of the advantages gained in design.

In regard to the few, and (at present) rare, metals rivalling aluminum in lightness, such for instance as magnesium, which is perhaps the most common, it is not likely that they can become its rivals for constructive purposes. This is not only on account of their physical characteristics, but also because their ores are probably not so plentiful. What may, however, be done with some of their alloys, we have no data for predicting. Alloy-making is an art in which lies hidden countless capabilities. Indeed, when we consider how the various metals may be combined, both as to number and to quantity, and how easily they are affected by the admixture of non-metallic substances—as, for instance, carbon in steel—the changes that may be rung to produce new and useful alloys seem almost infinite in number.

In predicting further improvements in our buildings, a safe field for flights of fancy is that of automatic conveniences of many kinds, which will undoubtedly be contrived to minister to our comfort and happiness. Among these

will probably be: still better elevators than now; arrangements for water-proof walls, floors and ceilings which can be perfectly purified and cleansed by water or steam jets; automatic heating and cooling arrangements to secure constant temperature and healthful ventilation; perfect control of lighting, to any degree and from various locations; the opening of doors, windows, shutters, shades, mosquito-nets, and other movable devices, by the mere touching of electric buttons, either in the immediate vicinity of the devices named, or from distant parts of the building; easier food-transmission and table service; inter-house telephones and other signals; automatic indicating, in every room, of time, temperature in-doors and out; barometric pressure, direction and force of wind, weather probabilities, etc. Another great desideratum in the future house will probably be obtained by its being made absolutely fire-proof, a result the value of which, to our nerves and to our purses, it is unnecessary to mention.

In the matter of *warmth*, which word I use advisedly, rather than heat, because I wish it to include a certain degree of coolness, there is certainly an inviting field for our future inventors. This subject was touched upon when speaking of buildings, but the improved methods therein desirable would also be required for making comfortable ships, railway cars, carriages, etc. That our present methods of heating by the burning of coal and the non-burning of smoke, and of cooling by carrying in lumps of ice, are crude and wasteful, as well as extremely irregular and uncomfortable, it is not necessary to argue about. It seems probable that in the near future, at any rate for our cities, some system of gas heating will displace coal and wood to a large extent, and this is the more likely from the fact that our streets and houses are already supplied with the necessary pipes, which may perhaps be gradually thrown out of use, as conveyers of illuminating gas, by the improvement and development of electric lighting. There also seems a tendency among inventors in recent years to attempt some kind of artificial cooling by the distribution through pipes of cold air or freezing mixtures of various kinds; also to



contrive cheap ice-making, or other cooling, machines which can be economically used in individual buildings.

All this is in a direct line of progress, but it seems probable that at some time in the future we shall depend upon the electric current brought into each building through a single wire, not only for our lighting and power, but for our heating and cooling also. Just how the latter process is to be accomplished we do not yet know, but, reasoning by analogy, it would not seem outside the range of possibility, when we consider the fact that the burning of coal in a furnace can be made to produce heat or cold at will. It is true that while electric heating in itself has been proved to be perfectly practicable, yet at present it is not economical. This is owing to the fact that our best and largest steam engines by which we run our dynamos utilize only from ten to fifteen per cent. of the energy stored in the coal, lavishly wasting the remainder. Fortunately, our dynamos have reached an efficiency of about ninety per cent., so that there is not much waste in using the electric current after the power to produce it is once generated. It is probable, therefore, that the use of electric heating will for the present be somewhat limited, and confined to special places where its convenience will offset its extra cost. For an extension of its employment thus, as for certain other important uses of electricity, and upon a scale far beyond anything we can now imagine, we must wait until we learn how to produce this current of pure energy from the coal or other fuel direct. Than the accomplishment of this feat, there is to-day no more fascinating problem in the realms of science. Many are working at it, with as yet but small success. We need not despair, however, as it seems to be a logical possibility, and the only thing required is to find out how to do it.

While upon the subject of warmth and coolness, the question naturally arises, shall we ever be able to control our climates, out-of-doors as well as in? At present we can conceive no possible way of doing this, and yet some youth among us, who should positively say that it is an impossibility, might live to realize that it is not well to be too prophetic. We already do something in the way of con-



trolling rain-fall, by regulating forests and inland seas. Practical experiments are about to be tried with explosions high in the air. Who knows what may happen when we learn more about atmospheric electricity?

Regarding *light*, the next subject of my classification : Dame Nature probably has many wonders in store for us, which, so far, it has not pleased her somewhat coquettish majesty to reveal. It is hardly likely that many very brilliant inventions will be made in pine-torches, tallow-dips, kerosene lamps or gas-burners, as the vast superiority of the electric light, even in its present crude state, bids fair soon to abolish all of those antiquated luminaries—especially in and near our cities, and within the range of their electric circuits. The most probable field that occurs to me for inventions of this class is in the way of solidifying petroleum or other cheap illuminants—thus making them more portable. Something I believe has already been done in this line, but how much I know not. Could such a process be made commercially practicable, and could there be some simple method of burning the product with a brilliant steady light, without smoke and without the nuisance of glass chimneys, we should have an ideal light for portable purposes and remote situations. Analogous improvements in tallow and whale-oil are scarcely to be expected, as these materials can never compete with petroleum in quantity and cheapness.

In regard to the improvement of the electric light itself, there does not seem to be a very large field ahead with the two present types that are in practical use, viz: the arc lamp and the incandescent, or "glow" lamp, as our English cousins more appropriately term it. Both of these devices have apparently reached a tolerable degree of perfection, but, so rapid is the development of electrical science, that we must prepare our minds for some radical change of methods at any time. It is well known that there is an excessive loss of energy in both of these systems, even after the steam engine has squandered nine-tenths of its patrimony. Especially is this the case with the glow lamp, which takes nearly ten times as much power, for a given amount

of light, as does the electric arc. The loss of energy in question is obviously due to its dissipation in the form of heat. If, therefore, we can imitate the oft-quoted glow-worm, and view the electric energy in the form of pure light, we shall enormously cheapen this boon of mankind. Many busy, practical brains are now working in this field; and others are approaching it from an abstract scientific standpoint, by trying to discover the inward nature of the mysterious forces known to us as electricity and magnetism. That glorious results will sometime come to us; who can doubt? When they do come, who can predict how great will be the impetus given to manners and to morals, to learning and to art, by a light so cheap that every farm and roadside, in common with every nook and corner of our cities, may, at will, be made to glow with the radiance of day?

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## NEW ALLOYS AND THEIR ENGINEERING APPLICATIONS.

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BY F. LYNWOOD GARRISON.

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*[A lecture delivered before the Franklin Institute, January 30, 1890, and February 6, 1891.]*

MEMBERS OF THE FRANKLIN INSTITUTE, AND LADIES AND GENTLEMEN :

In dealing with the subject of alloys, we have to consider a class of compounds about which, strange as it may seem, we know very little. Although the practice of combining or mixing one metal with another is older than history itself, it cannot be said that at the present time we have any established general laws regulating the subject as a whole. Those who have to do with alloys, generally determine by actual experiment or experience what mixtures and proportions give them the best results for specific purposes. The art of alloyage may therefore be considered as empirical, having little or no basis on scientific fact. I am, of course, speaking in very general terms, as there are many alloys whose properties and peculiarities have

been observed and determined with the greatest scientific care. The subject is a very extensive one when we take in consideration the numerous and varied iron alloys, which, to my mind, constitute the most difficult part of metallurgy.

The varying proportions of the elements present in an alloy are apt to produce such subtle effects that one must always be ready for the unexpected. Again, the properties of an alloy may be much altered by physical means; thus, for instance, the results that a given alloy will produce at ordinary temperatures may be very different from those it will give when heated or cooled. The treatment an alloy has received, such as shocks, strains, etc., will change its qualities to a greater or less degree; the manner in which it has been cast, cooled, rolled or hammered also affects its properties.

Alloys are interesting from a purely scientific standpoint; they are not only mixtures of metals having certain particular qualities, but also in many cases appear to be actual chemical compounds. In the phenomena which accompany the union of some of the metals we observe reactions which characterize the manifestation of affinity, such as an evolution of light and heat, resulting in the formation of substances having a definite composition, distinct crystalline form, and a variety of properties different from those of the constituents. Ordinarily, alloys may be considered to be simple mechanical mixtures of metals or solutions of one or more metals in another. It would also seem probable that certain proportions of one or more of the constituent metals may form definite chemical compounds with each other.

In using the term "new," in the title of these lectures, I mean that it shall be construed broadly, as I am well aware many of the alloys which I shall take up are not strictly new things. Their manufacture, composition and peculiarities are not sufficiently well known, however, to be considered old, although the extent of their use in the arts may be considerable.

I therefore feel it incumbent upon me to mention a few

of them at length, although in some cases they have long since been replaced by newer and better compounds.

The subject we have under consideration is so extensive that I find it will be necessary to divide it into two parts. In the first part, I purpose to confine myself to the copper alloys, such as the bronzes, brasses, etc., and in the second part to iron alloys, or more particularly to the combinations of steel with the various elements, such as nickel, copper, chromium, etc.

The rapid substitution of metals for wood and stone in various engineering constructions, the use of bronzes and other strong elastic alloys in place of cast or wrought iron; or, in short, the vastly more general use of metals in the arts, demands that engineers of all classes shall give much closer attention to the physical properties of these metals and metallic compounds than formerly. It is a mistake for engineers to follow rules of thumb; or, indeed, to adhere rigidly to any rules whatever in this particular. The problem to determine just what particular and peculiar kind of alloy is best suited for a given purpose appears to be a difficult one. The engineer should be governed to a great degree by his judgment, which, to be keen and correct, must be based upon a thorough knowledge of the fundamental laws of physics and chemistry. I am decidedly of the opinion that this most important matter (the physical properties of metals) receives insufficient consideration at the hands of the engineering professions; indeed, it is not improbable that many engineering disasters are due to our ignorance on this score. I cannot see that an establishment or individual is any better off when provided with a testing machine and a chemical laboratory if the results thereby obtained are not properly understood and applied.

Except as a matter of general information, a knowledge of the processes of smelting and refining metals can be of little use to the engineer who is not especially engaged in such work. A knowledge of the physical properties of metals, however, is of the greatest importance. Tensile strength, elastic limit, elongation, reduction of area, etc., should be to him as so many letters of an alphabet, whose

relation to each other in one aspect would mean a very different thing from their relation in another. In like manner, the chemical composition and the effect which the concomitant elements exercise upon the properties of the alloy should be understood. I must state, in connection with this, that, throughout this discourse, I use the word alloy in the broad sense, *i. e.*, a metallic mass composed of various metals in any proportion whatever; thus, for instance, I consider ordinary steel as much an alloy as brass.

I think there is nothing that can more forcibly demonstrate the importance of the subject, than the consideration of the advantages gained by the substitution of mild steel for iron, as, for instance, in the art of ship-building, where the advantages of the use of mild steel scantlings in place of iron is represented by a saving of twenty-five per cent. in the cost of fuel necessary to propel the ship.

The natural and proper tendency of engineers in designing structures is towards a decrease in weight, and at the same time an increase in strength. In order to meet this demand, the metallurgist must produce a material giving greater strength and durability in every particular—and to produce such results it is with these new alloys he will have to deal.

For the sake of convenience, I have divided the alloys I purpose to consider into two general classes, the copper alloys and the iron alloys. While in many instances the same elements are met with in both classes, the proportions are very different, and the system of manufacture, the uses and the peculiarities of the two classes are quite unlike.

#### COPPER ALLOYS.

*Copper Alloys (Bronze).*—Under this head we have to consider the copper-tin alloys constituting the bronzes proper, the copper-zinc alloys constituting the brasses, and the combinations of the bronzes and brasses with each other and with other elements, such as iron, manganese and phosphorus, in all cases the proportion of copper being in excess of the combined amounts of the other metals present.

The term bronze is usually, and by virtue of long usage,



properly applied to alloys of copper and tin; it is sometimes, however, improperly given to copper alloys containing little or no tin.

How, when and where the admixture of tin with copper to produce bronze was first discovered, is something we cannot hope to ascertain, for history gives us no clue to the mystery. As far back as we can trace, the use of bronze seems to have been common, and its fabrication, both by casting and forging, well understood.

The tools of the ancient Egyptians were generally made of bronze containing twelve parts of tin to eighty-eight parts of copper, doubtless hardened and toughened by hammering. The usual composition of the ancient bronzes was nine parts copper and one of tin; but the proportion of the latter varies from five to fifteen per cent., and in some cases lead has been detected. The use of lead in many of the old Roman bronzes appears to have been somewhat common, and probably it was added to increase their fusibility; it is true also that lead is an economical substitute for tin.

It is a remarkable fact, that even with our present scientific knowledge, we could hardly suggest an improvement upon the general composition of these ancient bronzes; or, with our advanced mechanical practice, surpass the admirable workmanship and beauty of form of some of the various implements of the developed bronze period.

Although the term brass appears in several places in the Old Testament, it probably did not mean an alloy of copper and zinc as we now understand it. The production and use of metallic zinc is comparatively modern, its existence and use as such does not appear to have been known at the time of Pliny, nor indeed at a very much later period. The Romans, according to Pliny, appear to have had the *trick* of giving their bronzes a golden color, by the addition of more or less of the mineral calamine (silicate of zinc— $\text{Zn}_2\text{SiO}_4$ ).

The zinc which found its way into their bronzes was probably by this means, or by the use of some other zinc ore which may have been mixed with the original copper ore, or added subsequently to the copper.

The composition of some of the English brass, of the year 1504, was found on analysis to be—copper, 64 per cent.; zinc,  $29\frac{1}{2}$ ; lead,  $3\frac{1}{2}$ ; tin, 3 per cent.

*Aluminum Bronze.*—Commercial pig copper almost invariably contains more or less dissolved cupreous oxide and occluded gases. The presence of these impurities tends to decrease the ductility of the metal, and their removal produces an astonishing increase in the tensile strength and general ductility.

In this simple fact lies the secret of the excellent results obtained with the improved copper alloys, known as aluminum bronze, phosphor bronze, manganese bronze, deoxidized copper, tempered copper, etc.

Aluminum, phosphorus, manganese, arsenic, silicon and, possibly, iron, when added in various ways to copper, act on these traces of cupreous oxide dissolved in the copper, deoxidizing the same and thus improving and toughening the copper. The slag, containing the oxides of the copper and the other metal or metalloid, resulting from this deoxidation, rises to the surface and can readily be removed.

This is probably the *rôle* aluminum plays in the manufacture of aluminum bronze. Truly it is not such a very remarkable one, when we consider there are nearly a half-dozen other elements which will do exactly the same thing.

Aluminum enthusiasts would have me believe that this property of producing an exceptionally strong ductile alloy, when aluminum is added in certain proportions to copper, is peculiar to aluminum—that such is not the case I hope to prove to you further on.

I cannot, in connection with this, refrain from mentioning the great amount of trash and nonsense which is being continually published about aluminum; if anyone will take the trouble conscientiously to examine into the subject, I think he will find that the field for the application of aluminum, at least in the engineering arts, is not particularly large. To quote one of the best criticisms I have yet seen on the subject: “Its (aluminum) diffusion,

difficult extractibility and remarkable properties seem to have appealed especially to that class of persons whose imaginative powers are largely in excess of their intellectual faculties."\* It is not my wish to try to belittle the peculiar and exceptionally valuable intrinsic properties of aluminum, but at a time when so-called processes for its production are being devised and published by the score, it is proper that I should warn engineers and capitalists to be on their guard against charlatans who have invaded this field. There are a number of excellent processes which have been worked out in a scientific manner, such as Castner's, Hall's, Bernard's, and several others, but the most of those we hear about in the newspapers are worthless catch-penny schemes of ignorant or designing people.

Aluminum bronze usually gives the best results when the proportion of its constituents are about ninety per cent. copper and ten per cent. aluminum. With small bars cast in sand, perhaps the best physical test results obtained were: elastic limit, 70,000 pounds per square inch; tensile strength, 95,000 pounds per square inch, with about ten per cent. elongation. With rolled bars much better results have been obtained, more particularly as regards elongation. The modulus of elasticity of aluminum bronze is about 18,000,000 pounds. Specific gravity when cast about 7.56,† when rolled about 7.89.‡

It must not be forgotten, however, that these are *best* results, and that the average run of ten per cent. aluminum bronze will give results somewhat lower than these, and on analysis will very probably be found to contain less than ten per cent. aluminum. This latter fact, however, is only what might be expected if the *rôle* which I have assigned to aluminum is the correct one.

The following analysis of aluminum bronze was made from the material used for firing pins by the Colt Fire Arms Company, of Hartford, Conn. I am indebted for this analysis

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\* *Engineer*, January 3, 1891, p. 1.

† Cowles Bros.' alloys.

‡ Hunt, *Trans. of the Am. Inst. of Mining Engineers*, vol. xviii, p. 555.

and also for a number of others of different alloys, to Mr. James S. de Benneville, of Dr. F. A. Genth's laboratory :

	I. Per Cent.	II.
Copper, . . . . .	91.260	91.260
Aluminum, . . . . .	7.410	7.410
Silicon, . . . . .	0.930	0.930
Iron, . . . . .	0.220	0.301
Lead, . . . . .	0.570	0.470
Phosphorus, . . . . .	0.056	0.025
Arsenic, . . . . .	0.042	trace
	<hr/> 100.488	<hr/> 100.396

The silicon and iron were probably present as impurities in the aluminum. I cannot, however, account for the presence of the lead, phosphorus and arsenic. Very small amounts of lead, however, are sometimes found in commercial aluminum.\*

The structure of aluminum bronze is close and dense, and although generally supposed to be workable at a red heat as easily as wrought iron, such, according to competent authorities, is not the case.† It can, it is true, be wrought at a red heat, but according to Richards, it possesses certain peculiarities at that temperature which must be strictly observed.

The melting point of aluminum bronze varies somewhat with the amount of aluminum contained, the higher grades melting at a somewhat lower point than the lower grades. Aluminum bronze shrinks more than ordinary brass. As the metal solidifies rapidly, it is necessary to pour quickly and to have the feeders large. Care must be taken to avoid the oxidation of the liquid metal in transferring to mould.

Aluminum bronze is applicable for a large number of purposes, such as blast-furnace tuyeres, various parts of marine engines, propellers, etc. Also for the firing pins of rifles, and parts of similar structures which are subjected to shocks.

\* *Trans. of the Am. Inst. of Mining Engineers*, vol. xviii, p. 530.

† *Richards' Aluminium*, p. 429, Philadelphia, 1890.

*Aluminum Brass.*—Aluminum bronze when added to ordinary brass greatly improves the brass in strength, toughness and resistance to corrosion. According to Messrs. Cowles\* “the greater the amount of aluminum bronze added to brass the harder and stronger the brass becomes. Tests have been made where castings of aluminum brass have given as high as 96,000 pounds per square inch in tensile strength.” It will be observed the elongation is not stated; it is not a difficult thing to make an alloy of even greater tensile strength, but the elongation will probably be *nil*, and the material so hard and brittle as to be worthless. Messrs. Cowles recommend aluminum brass for the following purposes:†

“Valve and valve-seats for mining pumps, or pumps working under great pressure, worms and worm-wheels, slide-faces, mining machinery, pinions, screws, hydraulic machinery, dredging machinery, gates for turbine wheels, working under a high head, propeller wheels, and as a metal to resist the action of salt water in marine architecture. While aluminium brass is not quite as strong or as tough as the A grade bronze, yet it will answer nearly as well for many purposes that the bronze is used for.”

Aluminum brass is stated to have great toughness, rigidity and strength.‡ The ordinary grades have about 85,000 pounds per square inch in tensile strength, with nine per cent. elongation. It is stated to be able to withstand a high temperature without loss of strength.

Recently some interesting experiments were made in the department of aerial navigation at Chalais, Charente, France, which produced some surprising results.§

Alloys of the following composition were made and rolled into sheets one mm. thick; from these, strips five mm. in width were cut and tested.

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\* Pamphlet on *The Alloys of Aluminum and Silicon*, p. 26.

† *Ibid.*, p. 25.

‡ *Iron Age*, May 2, 1889, p. 660.

§ *Engineer*, January 2, 1891, p. 2.



<i>Al.</i> Per Cent.	<i>Cu.</i> Per Cent.	<i>Sp. Gr.</i> Calculated.	<i>Sp. Gr.</i> Determined.	<i>Tensile Strength</i> <i>in Pounds per</i> <i>Square Inch.</i>
100	—	—	2·67	26,535
98	2	2·78	2·71	43,563
96	4	2·90	2·77	44,130
94	6	3·02	2·82	54,773
92	8	3·14	2·85	50,374

An interesting peculiarity of these alloys is the large divergence between the specific gravities calculated from those of their constituents and the specific gravities directly determined. Each two per cent. of copper might be expected to raise the specific gravity by 0·12, whereas the actual observed increase is only about 0·05.

It will also be observed that the addition of only two per cent. of copper increases the tensile strength from 26,535 to 43,563 pounds per square inch, while six per cent. more than doubles it. Thus it appears that an alloy of aluminum having double the tensile strength of aluminum itself can be made which is less than one-twentieth heavier.

*Manganese Bronze.*—This alloy has been used very extensively for casting propeller blades both in this country and abroad. The best results that I have yet seen obtained with it were the tests of some of the material made by Messrs. B. H. Cramp & Co., of Philadelphia. I have here a number of samples of this manganese bronze, which has been forged, rolled and hammered, both hot and cold. This material when cast in sand has an average elastic limit of 30,000 pounds per square inch, tensile strength of about 60,000 pounds per square inch with an elongation of eight to ten per cent. When rolled the elastic limit is about 80,000 pounds per square inch, tensile strength 95,000 to 106,000 pounds per square inch, and an elongation of twelve to fifteen per cent. These results are obviously excellent, and it is hardly to be expected they could be uniformly obtained, as in ordinary every-day foundry practice sufficient care is not taken to obtain the very best results. They show us, however, that much may be expected of this alloy, and that aluminum bronze is not the only copper alloy which will give us over 100,000 pounds per square inch in tensile strength.

For several years past, manganese bronze appears to have been made in large quantities by Mr. P. M. Parsons, of the Manganese Bronze Company, Deptford, England. Dr. Percy was probably the first to observe the action of the manganese in combining with the traces of cupreous oxide of copper present in the copper, deoxidizing the same, and thus making the metal denser and stronger.

Mr. Parsons, I believe, adds the manganese in the form of ferro-manganese. A portion of the manganese in the alloy thus added is utilized in the deoxidation above-mentioned, while the remainder, together with the iron, becomes permanently combined with the copper. The manganese once alloyed with the copper is not driven off by remelting, but usually the quality of the bronze is improved by a subsequent remelting.\*

The Manganese Bronze Company, of Deptford, England, roll and forge the alloy hot. According to Mr. Parsons,† its mean tensile strength as delivered from the rolls is about 67,200 pounds per square inch, with an elastic limit of 49,000 to 51,000 pounds per square inch, and an elongation of from twenty-three to twenty-five per cent. In cold rolling its ultimate tensile strength rises to about 90,000 pounds per square inch, with an elastic limit of 67,200 to 76,000 pounds per square inch, and an elongation of ten per cent. If annealed, the ultimate tensile strength is very little altered, but the elastic limit is reduced about half, and the elongation increased to thirty or thirty-five per cent.

In order to arrive at the practical value of an alloy for engineering purposes, it is necessary not only that it should be subjected to actual use for a length of time, but it is also

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\* It is interesting in this connection to find that about the year 1868, Messrs. Montefiore, Levi and Künzel, two Belgian chemists, made a number experiments with copper and manganese alloys, and from their results concluded such alloys to be useless. "They obtained great tensile strength and toughness with some of the compositions, but their ready oxidation at high temperatures made the qualities of the castings uncertain and impracticable."—*Journal of the Society of Arts*, May 7, 1877, p. 552.

† *Engineering*, April 20, 1888, p. 398.

necessary that all the strains and conditions under which it is used should be understood as far as possible. Thus, for instance, in the matter of propeller blades, transverse strength has to be considered above tensile, as the first named is the strain to be more particularly provided against. In this strain two forces come into play, tension and compression, but neither of these, separately, gives any indication of the transverse strength of the metal.

The following tables of transverse tests will be found interesting in connection with what has preceded. Nos. 1 and 2 were made at the works of Messrs. Maudsley, Sons & Field, by direction of the British Admiralty.\*

A bar of manganese bronze one inch square, laid on supports twelve inches apart, required on an average about 5,000 pounds, applied in the middle, to break or pull it through the supports, as against 2,500 pounds for gun metal.

The above-mentioned bar of manganese bronze (Parsons' No. 2) was made by Parsons' method, at the Manganese Bronze Company's works, at Deptford, London.

The Bronze of Nos. 3 and 4 was made at the works of Messrs. B. H. Cramp & Co., Philadelphia, and the tests at the New York Navy Yard.†

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\* *Engineering*, April 20, 1888, p. 398.

† Private communication from Messrs. Cramp & Co.

I.

GUN METAL.			Resili- ence.
Strain Applied.	Deflection under Strain.	Perma- nent Set.	
<i>pounds.</i>	<i>inches.</i>	<i>inches.</i>	<i>inches.</i>
800	'03	—	—
1,000	'045	—	—
1,200	'06	'015	'045
1,400	'09	'04	'05
1,600	'15	'09	'06
1,800	'27	'20	'07
2,000	'46	'38	'08
2,200	'75	'65	'10
2,400	1'08	'97	'11
2,600	1'59	1'46	'13
Slipped through supports. Perma- nent deflection, 3 $\frac{5}{8}$ inches.			

A steel bar subsequently tested broke with 5,600 pounds, the total deflection being about  $\frac{3}{4}$  inch.

II.

MANGANESE BRONZE.			Resili- ence.
Strain Applied.	Deflection under Strain.	Perma- nent Set.	
<i>pounds.</i>	<i>inches.</i>	<i>inches.</i>	<i>inches.</i>
800	—	—	—
1,000	—	—	—
1,200	—	—	—
1,400	'06	—	—
1,600	'07	—	—
1,800	'08	'005	'075
2,000	'09	'01	'08
2,200	—	—	—
2,400	—	—	—
2,600	—	—	—
2,800	—	—	—
3,000	'20	'075	'125
3,200	'23	'105	'125
3,400	'28	'140	'140
3,600	'34	'19	'15
3,800	'41	'26	'15
4,000	'49	'33	'16
4,200	'59	'425	'165
4,400	'7	'53	'17
4,600	'81	'63	'18
4,800	1'16	'96	'20
5,000	1'39	1'17	'22
5,200	1'7	1'47	'23
Fractured, permanent deflec- tion 3 3-16 inches.			

The following transverse tests of Cramp's manganese bronze were from the pouring gate for the propeller hub of the U. S. S. *Maine*, Navy Yard, New York, May 16, 1890.

Ten inches between supports. Loads applied on the centre. On specimen III the loads were applied and allowed to remain two minutes before the deflection was measured, after which the loads were removed and the recovery measured. The above operations were repeated

with each load until 3,750 pounds were applied, when the deflection was so great as to allow the specimen to rest on the edges of the blocks, after which the recovery was not measured, and the load was increased until the specimen broke. Specimen IV was tested, as shown in the table:

## III.

Placed in position measured 10'562". Section of III 1'009 by 1'013".

Lbs. Total.	LOAD.			
	Spec. Measured.		Spec. Deflected.	
	On.	Off.	On.	Off.
500	10'527	10'562	0'035	none
700	10'520	10'562	0'042	none
800	10'517	10'562	0'045	none
900	10'509	10'562	0'053	none
				Perm. set.
1,000	10'507	10'555	0'055	0'007
1,300	10'484	10'545	0'078	0'017
1,400	10'480	10'540	0'082	0'022
1,600	10'460	10'532	0'102	0'030
1,800	10'437	10'517	0'125	0'045
2,000	10'378	10'485	0'184	0'077
2,200	10'358	10'478	0'204	0'084
2,500	10'256	10'382	0'366	0'180
2,750	10'140	10'290	0'422	0'272
3,000	9'8'10	9'995	0'752	0'567
3,500	9'286	9'485	1'276	1'077
3,750	8'689	—	1'873	} 9" betw. sup'rts.
6,300	8'540	Broke.	2'022	

## IV.

Placed in position measured 10'562". Section of IV 1'007 by 1'0115".

Lbs. Total.	LOAD.			
	Spec. Measured.		Spec. Deflected.	
	On.	Off.	On.	Off.
1,150	10'507	10'556	0'055	0'026
2,000	10'404	10'523	0'158	0'029
2,500	10'247	10'400	0'315	0'162
3,000	9'9'18	10'083	0'644	0'479
3,500	9'598		0'964	} 9" betw. sup'rts.
6,400	8'079	Broke.	2'483	

Specimen.	APPLIED LOAD.		Deflection in Inches.	Sp. Gr.	Hardness.	Bent before Breaking.	REMARKS.
	Total.	Maximum Fibre Strain.					
3	6,300	82,150 *	2'022	8'24	1'52173	24°-07"	
4	6,450	84,530 *	2'483	8'18	1'6279	28°-48'-30"	

\* From formula  $f = \frac{3}{2} \cdot \frac{W' l}{b h^2} \cdot l \quad 9''$



## TENSILE TESTS—CRAMP'S MANGANESE BRONZE.

*Cast in Sand.*

	Size Diameter.	Area in Square Inches.	Tensile Strain, Pounds per Square Inch.	Limit of Elasticity in Pounds per Square Inch.	Elongation. Per Cent.	Reduction of Area.
1	'631	'3127	60'154	33'579	12'5	9'6
2	1'00	'7854	48'510	26'730	9'2	13'5
3	'645	'32674	63'735	34'278	6'0	10'5
4	'749	'4106	59'910	34'040	11'6	18'7
5	'6234	'30514	63'249	39'326	2'5	3'5
6	'746	'4370	63'730	37'2.0	7'6	11'9

*Rolled Bars.*

7	'636	'317	106'056	82'019	17'	21'5
8	633	'3197	98'340	86'645	14'	25'7
9	'743	'4336	93'200	59'960	21'2	28'1
10	'744	'4347	85'410	64'870	20'	23'6
11	'634	'3157	101'299	88'850	10'5	17'7
12	1'004	'7917	94'250	87'150	8'6	14'1

*Compression tests of Cramp's Manganese Bronze (cast in sand).*—Tests were made at United States Navy Department from the metal in the pouring gate of propeller hub of U. S. S. *Maine*. These test pieces were cut from the specimens which had already been tested to destruction under transverse stress, and the results are undoubtedly lower than would be shown by metal which had not been previously strained.

The specimens were 1 inch high by 0.7 x 0.7 inch in cross-section = 0.49 square inch.

Both specimens gave way by shearing, on a plane making an angle of nearly 45° with the direction of stress.

	Load Applied.	Crushing Stress per Square Inch.
A, . . . . .	61,950 lbs.	126,450 lbs.
B, . . . . .	66,500 "	135,750 "

A subsequent test on a specimen 1 x 1 x 1 inch was made from a piece of the same pouring gate, but which had not been previously strained. Under stress of 150,000

pounds it was flattened to 0.72 inch high by about  $1\frac{1}{4}$  x  $1\frac{1}{4}$  inches, but without rupture or any sign of distress, showing the interior to be homogeneous and without flaws.\*

*Torsion tests of Cramp's Manganese Bronze (cast in sand).*—The following is a copy of report to the Commandant of New York Navy Yard:

NAVY YARD, NEW YORK, February 14, 1889.

SIR:—In obedience to your order of the 16th ultimo, a copy of which is hereto attached, we have tested for torsional strength two pieces of Vesuvius metal,† one rolled and one cast, and beg leave to report as follows:

The test specimens were turned to the form shown in the sketch furnished by the Bureau, and reduced to three-fourths and fifteen-sixteenths inch diameter between fillets, respectively.

The one cast while being reduced from one and one-fourth inches (diameter of the ends) to fifteen-sixteenths of an inch, developed a number of small blow-holes for the entire length between fillets.

Some of these were nearly cleaned up by the finishing cut, while others were just developing, their depth varied from one-sixty-fourth to one-thirty-second of an inch.

After being turned to their respective sizes, levers of forty-eight and eight inches, respectively, were keyed on each end. The test pieces were then put on the centres of a heavy lathe, the long lever towards the tail stock, and the short lever towards the face plate end, the small lever brought up against a lug on face plate which securely held the piece in position.

A Z-bar was then bolted to the carriage of the lathe; about the centre of the long lever. A sliding plate, with a knife edge, working freely in a slot in the bar was brought up to the bottom side of the lever whilst in its horizontal position, the knife edge just touching it. This enabled the observer to sight between the knife edge and lever, and see

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\* Private communication.

† Cramp's manganese bronze.

that it was kept floating in a horizontal position for the required time.

On the side of the lathe opposite to where the weight was applied, and while the lever was horizontal, a zero mark was made on the periphery of the face plate (diameter 46.26 inches) squared from the carriage of the lathe.

Before commencing test, 2.92 pounds was added to scale pan in order to bring the equivalent weight on end of lever up to ten pounds.

The increments were then added and the lathe turned slowly by hand, just sufficiently to keep the lever horizontal, by means of the back gear; another mark was then made on the periphery. At the end of two minutes the load was removed and another mark was made, and so on, for each increment.

When the amounts of torsion in the piece rolled was 3,120 ( $F = 37,717$ ), and in the piece cast 3,360 ( $F = 20,797$ ) inch pounds, the lever in each case supporting the weight but a short time and then slowly settled down on the knife edge; the same condition prevailed for the subsequent moments until the breaking stress was reached.

The elastic limit was not clearly defined, but the Board thinks that it occurred where the first permanent set showed itself, viz: in the piece rolled between moments of torsion 1,440 and 1,680 ( $F = 17,408$  and  $F = 20,309.4$ ), and in the piece cast between moments 1,920 and 2,400 ( $F = 11,884$  and  $F = 14,855$ ).

The calculations in connection with the tests, together with diagram showing curves of torsion and permanent set, are hereto attached.

Very respectfully,

J. J. BARRY, *P. A. Engineer, U.S.N.*

F. H. BAILEY, *P. A. Engineer, U.S.N.*

J. L. WOOD, *Ass't Engineer, U.S.N.*

*To the Commandant,*

*Navy Yard, New York, N. Y.*

Commandant's Office, Navy Yard, New York, approved  
February 15 1889.

F. M. RAMSAY,

*Captain Commanding.*

Vesuvius Metal* Cast.	Description of Test Piece.		Number.		Distance of Mark on Face Plate from Zero. Load on.		Distance of Mark on Face Plate from Zero. Load Removed.		Torsion Angle from Zero. Load on.		Torsion Angle from Zero. Load Removed.		Twist of Piece Measured on Circle 12" Radius.		Moment of Torsion-Inch-pounds.		Value of $F$ from $M = \frac{f d^3}{5.1}$ $F = \frac{48 \times W \times 5.1}{9375^3}$		Angle of Recovery.	
	Diameter of smallest Part.	Length between Fillets.	Length Lever between Centre and Point where Wt. is Applied.	Moment of Lever alone in Inch-pounds.	Limit of Elasticity in Inch-pounds.	Breaking Stress in Pounds per Square Inch.	0	10	0	0	0	0	0	0	480	2971	0	0		
	1.57	4"	4"	3401250	Between Moments of Torsion 1920 and 2400 ( $F = 11884$ and $F = 14855$ ).	53478	1	20	3-16	0	0	0	0	0	960	5942	46°	0		
							2	30	11-32	0	0	0	0	0	1440	8913	85°	0		
							3	40	9-16	0	0	0	0	0	1920	11884	1°-39	0		
							4	50	29-32	7-32	7-32	54°	469	2400	2880	14855	1°-70	0		
							5	60	17-16	17-32	17-32	10°-31	746	2880	17826	17826	2°-25	0		
							6	70	27-8	13-4	7°-11	4°-33	1489	3360	20797	20797	2°-78	0		
							7	80	55-8	45-16	13°-91	10°-67	2911	3840	23768	23768	3°-24	0		
							8	90	99-16	81-16	23°-65	19°-94	4953	4320	26739	26739	3°-71	0		
							9	100	145-8	13	36°-17	32°-15	7575	4800	29710	29710	4°-02	0		
							10	110	201-4	181-4	50°-08	45°-13	10489	5280	32681	32681	4°-95	0		
							11	120	2613-16	245-8	60°-31	60°-90	13389	5760	35652	35652	5°-41	0		
							12	130	341-8	319-16	84°-39	78°-06	17675	6240	38623	38623	6°-33	0		
							13	140	421-16	395-16	104°-02	97°-22	21786	6720	41594	41594	6°-80	0		
							14	150	5011-16	473-4	125°-35	118°-09	26253	7200	44565	44565	7°-26	0		
							15	160	611-8	58	151°-16	143°-43	31659	7680	47536	47536	7°-73	0		
							16	170	721-2	693-16	179°-29	171°-10	37540	8160	50907	50907	8°-19	0		
							17	180	779-16	Broke.	191°-81	Broke.	40179	8640	53478	53478	—	0		

\* The Cramps call their manganese bronze Vesuvius metal.

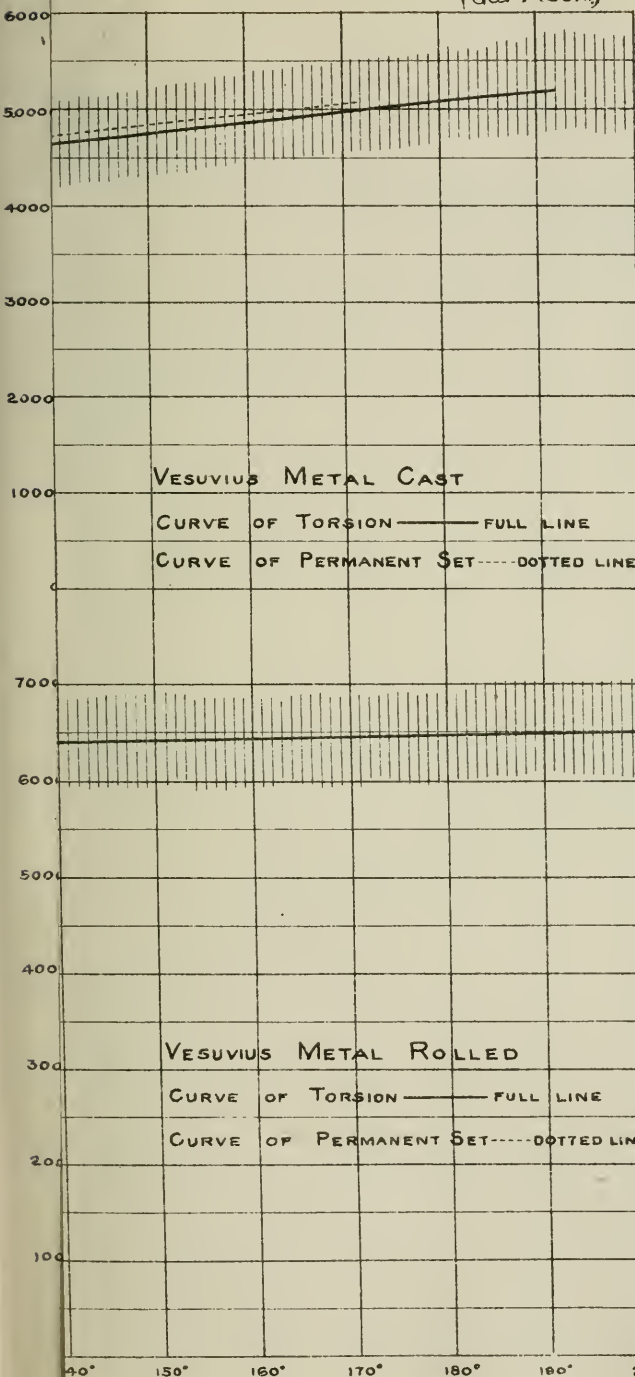
Description of Test Piece.	Diameter of smallest Part.	Length between Fillets.	Length Lever between Centre and Point where Wt. is Applied.	Moment of Lever Alone in Inch-pounds.	Limit of Elasticity in Inch-pounds.	Breaking Stress in Pounds per Square Inch.	Number.	Equivalent Weight on End of Lever	Distance of Mark on Face Plate from Zero. Load on.	Distance of Mark on Face Plate from Zero. Load Re-moved.	Torsion Angle from Zero. Load on.	Torsion Angle from Zero. Load Re-moved.	Twist of Piece Measured on Circle 12" Radius.	Moment of Torsion-Inch-pounds.	Value of $F$ from $M = \frac{51}{d^3} \times W \times 48$	Angle of Recovery.	
Vesuvius Metal * Rolled.	" 4	" 4	" 4	340.1250	† Between Moments of Torsion 1440 and 1880 ( $F = 17408$ and $F = 20309.4$ .)	66799	0	10	0	0	0	0	0	480	5802.0	5802.0	0
							1	15	3-16	0	0	0	0	720	87.40	87.40	0
							2	20	3-8	0	0	0	0	960	1105.0	1105.0	0
							3	25	17-32	0	0	0	0	1200	14506.7	14506.7	0
							4	30	3-4	3-32	10-31	23-06	0	1440	17108.0	17108.0	0
							5	35	15-16	5-32	20-85	0	1680	20309.4	20309.4	0	
							6	40	1	1-4	3-6	0	1920	23911.0	23911.0	0	
							7	45	1	7-16	3-56	0	2160	26913.0	26913.0	0	
							8	50	1	25-32	4-41	10-16	0	2400	29415.0	29415.0	0
							9	55	2	1-8	5-6	0	2640	31914.0	31914.0	0	
							10	60	2	9-16	6-34	20-32	0	2880	34815.0	34815.0	0
							11	65	3	1-4	8-64	30-56	0	3120	37717.0	37717.0	0
							12	70	4	11-32	10-74	50-87	0	3360	40619.0	40619.0	0
							13	75	6	3-16	15-30	10-05	0	3600	43520.0	43520.0	0
							14	80	9	1-16	22-74	16-05	0	3840	46421.0	46421.0	0
							15	85	13	1-8	32-06	26-05	0	4080	49323.0	49323.0	0
							16	90	17	31-32	41-44	37-72	0	4320	52224.0	52224.0	0
							17	95	23	5-8	58-43	51-24	0	4560	55125.0	55125.0	0
							18	100	30	1-2	75-43	65-03	0	4800	58026.0	58026.0	0
							19	105	38	5-8	95-53	82-04	0	5040	60927.0	60927.0	0
							20	110	54	3-4	135-41	126-43	0	5280	63828.0	63828.0	0
							21	115	139	5	343 77	Broke	0	5520	66729.0	66729.0	0

\* The Cramps call their manganese bronze Vesuvius metal.

† This Moment of Torsion ought to be 965. Giving for a value of  $F$  11605.

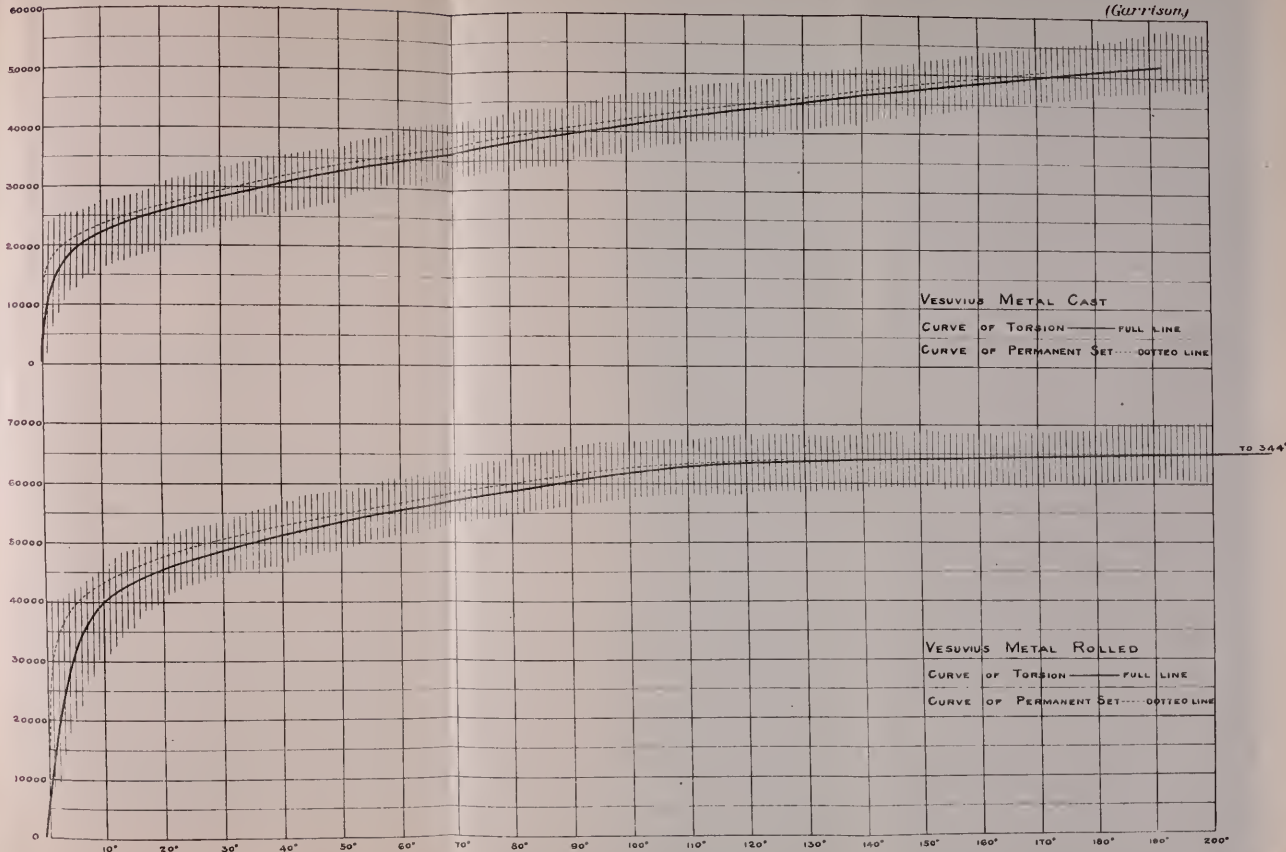


(Garrison.)



TO 344°

(Garrison)



The Directors of the Peninsular and Oriental Steamship Company some few years ago decided to fit one of their steamers with a set of manganese bronze blades, and in order to arrive at the correct value of the advantage of this metal over steel, it was decided to take off the steel blades that had been in use four years, the performance of which had been carefully noted, and replace them with bronze blades, cast by the Manganese Bronze Company, of Deptford, of the same diameter, pitch and area, and in every way duplicate, except thickness of material and fine cleavage edge. These blades were satisfactorily cast and fitted to the steamship *Balaarat*, from which the following results were obtained.

	Speed Knots.	Coal per Day, Tons.	I. H. P.	Slip Per Cent.
Steel Blades, . . . .	12'11	63'8	2'828	13'1
Bronze " . . . .	12'35	55'0	2'577	9'7

The advantages thus gained by the use of manganese bronze represent a saving of 700 tons of coal on the round voyage.\*.

One of the great objections to the use of manganese bronze, or in fact any alloy except iron or steel, for the propellers of iron ships is on account of the galvanic action set up between the propeller and the stern-posts. This difficulty has in great measure been overcome by putting strips of rolled zinc round the propeller apertures in the stern frames.

*Manganese Bronze Tubes.*—A manganese bronze tube, similar to those used in hydraulic gun carriages, was recently subjected to a severe test by Armstrong, Mitchell & Co., England. The tube was 4'9 inches outside diameter, 0'3 inch thick and 18 inches long. An internal pressure of 4,000 pounds per square inch was applied without increase of outside diameter; 5,000 pounds pressure produced an enlargement of 0'03 inch; 6,000 pounds pressure increased the diameter 0'08 inch. The testing pump could not go beyond this pressure, so the tube was turned off to an outside diameter of 5'55 inches, leaving it 0'125 inch in thickness. A pressure per square inch of 1,000 pounds produced

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\* *Trans. of the Institution of Naval Architects*, 1888, pp. 378, 379.

no change of diameter; 2,000 pounds pressure increased the diameter 0.1 inch; 2,500 pounds pressure burst the tube. Two longitudinal specimens were taken from the tube and tested for tensile strength. They gave an ultimate strength of 31.5 tons (70,560 pounds) per square inch and a stretch of  $27\frac{1}{2}$  per cent. in 3 inches. Some of the advantages claimed for tubes made from this metal are freedom from the pitting and corrosion experienced with steel, and a strength when annealed double that of the best copper. For the same strength manganese bronze tubes weigh about one-half that of copper or brass, and cost about one-third less per running foot.\*

Parsons makes five grades of manganese bronze, the first of these is used wherever strength is wanted, the fourth and fifth only for anti-friction purposes.

No. 1 is the only grade which cannot be cast in sand. It is used entirely in the forged, rolled and drawn condition. The ingots for this purpose are cast in chill moulds in the usual manner. As manganese bronze is but very slightly affected by the corrosive action of impure water, this grade is well adapted for propeller studs, piston rods, centrifugal pump spindles, etc. It is also well adapted for stampings of all kinds and for bolts of different sizes.

Parsons' manganese bronze No. 2 is designed for purposes where strength and toughness are required, but in which the form of the article necessitates casting, such as propeller blades, cylinders for ordnance, hydraulic rams, gun-mountings, etc.

The strength of No. 2 Parsons' manganese bronze cast in sand averages about as follows: elastic limit, 35,000 to 43,000 pounds per square inch; tensile strength, 63,000 to 75,000 pounds per square inch; elongation, from thirteen to twenty-two per cent. in five inches.

The following analysis of this No. 2 bronze was made from a chip off of the propeller of Mr. W. K. Vanderbilt's yacht *Alva*.†

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\* *Engineering and Mining Journal*, January 31, 1891, p. 146.

† This analysis was made by Mr. James S. DeBenneville, in Dr. Genth's laboratory.

Copper, . . . . .	88.644
Zinc, . . . . .	1.570
Tin, . . . . .	8.700
Iron, . . . . .	0.720
Lead, . . . . .	0.295
Phosphorus, . . . . .	trace
	<hr/>
	99.929

It will be observed there is no manganese present and the amount of zinc is very small. I have a number of analyses of different kinds of manganese bronze, and I note the presence of very small amounts of phosphorus in all. I am unable to account for its presence, unless it was originally in the pig copper.

Tests of transverse strength by impact of this grade (No. 2) of manganese bronze, of gun metal and of steel are as follows: The impact tests were made by a weight of fifty pounds falling on the middle of the bar from a height of five feet, the bar resting on supports twelve inches apart.

The manganese bronze broke with thirteen to seventeen blows, the deflection at the eighth blow averaging 3.60 inches.

The gun metal broke with seven to eight blows, the deflection after the fifth blow averaging 3.65 inches.

The steel broke with three blows, deflection 0.66 inches.\* The quality of steel is not stated.

The gun-metal bars were cast by Messrs. Maudsley, Sons & Field, of the strongest mixture that could be obtained. The steel bars were cut from the thin end of a propeller blade. It will be seen from the transverse experiments previously given that the manganese bronze has about twice the strength of the gun metal. It required one ton to give the manganese bronze bars a permanent set of .01 inch in twelve inches, whereas the gun-metal took a set of 0.15 inch with twelve hundredweight. It will also be observed that its resilience is about the same proportion, viz: nearly twice that of gun metal. It should be observed that although the steel bar required 2,000 pounds more to break it by steady pressure than the manganese bronze bar, it

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\* *Parsons' Manganese Bronze Catalogue*, 2d ed., p. 41.



was a long way behind when tested by impact, the work done in breaking the latter being much more than in the former.\*

In *Figs. 1, 2 and 3*, we have shown several manganese bronze propellers cast by Messrs. Cramp. They were hammered and bent cold under a steam hammer to test the ductility of the metal.

#### COPPER-ZINC-IRON ALLOYS.

*Delta Metal*.—This alloy, which was formerly known as sterro-metal,† is composed of about sixty parts of copper, from thirty-four to forty-four of zinc, from two to four of iron, and from one to two of tin.‡

The peculiarity of all these alloys is the content of iron which appears to have the property of increasing their strength to an unusual degree.§ In making delta metal

I	Per Cent.	II.	Per Cent.
Iron, . . . . .	0·1-5	Iron, . . . . .	0·1-5
Copper, . . . . .	50-65	Tin, . . . . .	0·1-10
Zinc, . . . . .	49·9-30	Zinc, . . . . .	1·8-45
		Copper, . . . . .	98-40

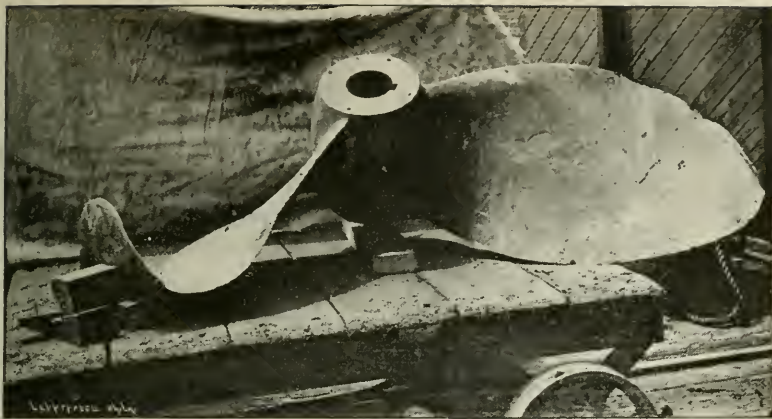
the iron is previously alloyed with zinc in known and definite proportions. When ordinary wrought iron is introduced into molten zinc, the latter readily dissolves or absorbs the former, and will take it up to the extent of about five per cent. or more. By adding the zinc-iron alloy thus obtained to the requisite amount of copper, it is possible to introduce any definite quantity of iron up to five per cent. into the copper

\* *Parsons' Manganese Bronze Catalogue*, 2d ed., p. 41.

† Percy's *Metallurgy of Iron and Steel*, p. 157.

‡ What is known as Aich's metal is practically the same thing.

§ Delta metal appears to be a patented compound, but as it is exactly the same composition as sterro-metal which was introduced many years ago by Baron Rosthorn, of Vienna, it is difficult to see upon what patentable claim it can be based. Percy mentions at length sterro-metal and similar alloys in his *Metallurgy of Iron and Steel*, published in 1864. I find Delta metal was patented by Dick, in 1882. (British Patent, No 2,484, 1882, amended December, 1889). According to the specification the alloy may or may not contain tin, and it may have the variable composition indicated above.



1



2



3

FIGS. 1, 2, 3.—Manganese-bronze propellers. Tested cold under the steam-hammer.

alloy. Hiorns\* states that the inventor (Mr. Dick) uses a small amount of phosphorus in combination with the copper to avoid the oxidation when the alloy is remelted. In some cases he uses tin, manganese and lead to impart special properties. The inventor claims that by his process the iron is chemically combined in the brass and bronze.

The advantages claimed for delta metal are great strength and toughness. It produces sound castings of close grain. It can be rolled and forged hot, and can stand a certain amount of drawing and hammering when cold. It takes a high polish, and when exposed to the atmosphere tarnishes less than brass.

When cast in sand delta metal has a tensile strength of about 45,000 pounds per square inch, and about ten per cent. elongation; when rolled, tensile strength of 60,000 to 75,000 pounds per square inch, elongation from nine to seventeen per cent. on bars 1.128 inch in diameter and 1 inch area.†

Wallace gives the ultimate tensile strength 33,600 to 51,520 pounds per square inch, with from ten to twenty per cent. elongation.‡

In *Fig. 4*, we have shown a composite propeller blade made by the Delta Metal Company, London. It consists of annealed cast steel, upon which is cast a comparatively thin outer coating of delta metal or bronze. It is claimed that in casting the delta metal on the steel the former becomes to a certain extent chilled, and then has its ultimate tenacity considerably increased. This combination is claimed to give the blade great strength to resist cross-breaking. It is stated that the adhesion of the brass to the surface of the steel has been found by actual tensile test to be about fifteen tons per square inch. The core is formed with a flange through which studs holding the blade to the boss pass, and the delta metal is dove-tailed with the steel in casting in such a way as to give great strength to resist fracture through

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\* *Mixed Metals*, p. 115. Macmillan & Co., London. 1890.

† *Iron* (London), vol. 21, 1883, p. 159.

‡ *Trans. of the Institution of Naval Architects*, 1888, p. 371.

the stud holes in the flange. To prevent any slight galvanized action taking place between the delta metal and the steel at the line of contact on the face of the flange and in the stud holes, there is used an insulating material which can readily be applied.\*

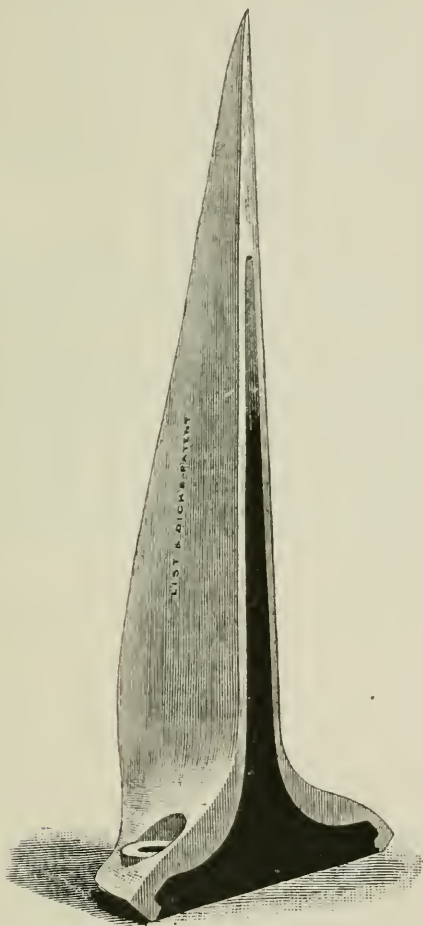


FIG. 4. Composite propeller blade of delta metal and steel.

Delta metal can be forged, stamped and rolled hot. It must be forged at a dark cherry red heat, and care taken to avoid striking when at a black heat.

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\* *Iron* (London), July 25, 1890, p. 72.

According to Lloyd's Proving House tests, made at Cardiff, December 20, 1887, a half-inch delta metal rolled bar gave a tensile strength of 88,400 pounds per square inch, with an elongation of thirty per cent. in three inches. The *Schweizerische Bauzeitung* states that the large worm wheels for the Pilatus Mountain Railway were cast in delta metal by the *Schweizerische Locomotivfabrik* in Winterthur. A test bar was cast with each piece, these bars were then tested with the following results: tensile strength 36.6 kgs. persquare mm., or 51,520 pounds per square inch; elongation, 10.5 per cent.\*

[To be continued.]

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## RIVETED JOINTS IN BOILER-SHELLS—LE VAN.

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The Publication Committee wishes to state, to the readers of the *Journal*, that Mr. Le Van has written to it, expressing regret at his inadvertence in not acknowledging the sources of parts of his article on "Riveted Joints in Boiler Shells," which appeared in the April and May impressions of this journal.

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\* *Engineering*, April 22, 1887, p. 382.



PROCEEDINGS  
OF THE  
CHEMICAL SECTION  
OF THE  
FRANKLIN INSTITUTE.

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*[Proceedings of the stated meeting, held Tuesday, May 19, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, May 19, 1891.

Dr. WM. H. GREENE, Vice-President, in the chair.

Sixteen members and seven visitors were present.

Dr. W. P. Mason, of the Rensselaer Polytechnic Institute, of Troy, N. Y., read a paper on, "Drinking Water and Disease." It was referred for publication. A discussion of the paper, which was of much interest, followed, and was conducted by the author and Messrs. Pemberton, Jayne and Haines.

Mr. Pemberton called attention to the conditions of the water supply of Chester and discussed the effect of Philadelphia sewage upon it.

Dr. Bruno Terne presented a paper on "Meat Preservatives," giving a discussion of the ingredients and their effects upon the human system when present in meat used as food.

The paper was discussed by the author and Mr. Haines.

Adjourned.

WM. C. DAY, *Secretary*.

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ON THE AMMONIA PROCESS OF WATER ANALYSIS.

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BY REUBEN HAINES.

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*[Read at the stated meeting, held April 21, 1891.]*

Two forms of this process are in use, one dealing with a half litre or a full litre of the water; the other requiring only 100 cc. for the distillation. The writer prefers the former, although some chemists find the latter fully as accurate and more convenient.

The following notes indicate the practice of the writer for a number of years, in addition to the instructions given by Wanklyn.

The amount of sample used is one-half litre, or, in some very impure waters, a less quantity of the sample diluted to half a litre with pure distilled water.

The retort is of large size, having a capacity of from one-third to one-half gallon when completely filled, and is supported at the neck by a large retort-clamp upon a heavy iron stand. It should not be supported on a ring, unless wire gauze be interposed, as otherwise there is great danger of fracture of the retort. If a Bunsen burner with wire gauze is used close up beneath the retort with such a body of liquid there is still danger of fracture.

The neck of the retort is inclined *upward* at an angle of about  $25^{\circ}$  and attached to a Liebig condenser having an inner tube of one inch diameter, by a short piece of wide rubber-tubing in such manner that the ends of the retort and condenser are in close contact and that as little rubber as possible is exposed to the action of the hot vapor. In lieu of this arrangement a few inches of the end of the retort-neck may be bent downward parallel with the condenser, and be made to enter loosely within the glass condenser tube; a short piece of rubber-tubing overlapping the connection. In either case, the rubber should be tied down to prevent leakage, but in such manner that the retort may readily be disconnected at the conclusion of the analysis. The rubber will have to be renewed after a time. The mode of connection recommended by Wanklyn has been shown by Dr. Smart, Mallet and others, to permit loss of ammonia by leakage.

A rather rapid current of water should be maintained in the Liebig condenser. If this condenser alone is used, the current should be so rapid that the temperature of the water at the exit is not materially raised by the operation if the water used has an original temperature of from  $50^{\circ}$  to  $60^{\circ}$  F. If the water is of higher temperature than this ice should be used for cooling it. A better plan is that adopted by the writer, which consists in attaching the lower end of the Liebig condenser as closely as possible to a glass vertical coil condenser of the Mohr pattern, of which the outer vessel is an inverted jar receiver (a large inverted bottle with the bottom cut off will do very well), into which broken ice may be placed, especially in hot weather. The glass tube forming the coil has an inside

diameter of about half an inch. Beneath this second condenser a short nearly horizontal tube, conducts the distillate to the Nessler glass out of reach of contaminating dust, etc., that might chance to fall from the vertical condenser.

By having the retort sufficiently large, and by inclining its neck upward all danger of the permanganate being thrown over into the condenser is avoided. Risk of fracture is greatly lessened by the use of the retort clamp, and by the rod of the stand acting as a sort of spring to moderate the shock of bumping, and the retort may thus last in frequent use for a very long time. One of mine lasted several years, but it became unavoidably weakened in time by the corrosive action of the boiling alkaline liquid. By the above-described arrangement of the condensing apparatus, the distillate may be obtained at a given uniform temperature in all analyses in order to get uniform depths of color in the Nessler testing, a precaution to which some one has called attention. The risk of loss by imperfect condensation of ammonia during regular distillation, or through the outrush of gusts of vapor produced by turbulent boiling is also avoided. The condensing tube should, on account of this latter circumstance, be of comparatively large diameter throughout its whole length. With the above arrangement I have not found any loss by imperfect condensation, if the temperature of the distillates did not exceed 60° F. If a lower temperature than this is adopted, the distillates should be allowed to stand until they acquire a uniform temperature before applying the Nessler test.

The arrangement I have adopted was suggested by reading the *Report on Water Analysis*, by Prof. Mallet, to the National Board of Health, 1882, in which this loss of ammonia occurring when Wanklyn's instructions are followed is referred to, and a somewhat different arrangement suggested for avoiding it. In experiments with a known solution of ammonium chloride this loss was found by Dr. Smart to amount to an average of about seven per cent. of the whole amount that should have been obtained. It should, however, be noted that Mallet states that "the temperature of the condensing water varied from 25° to 25°·5 C."

which is a rather high temperature. In these experiments the current of water in the condenser was so rapid that its temperature was not sensibly raised by the process, even when fifty cc. of distillate were obtained in eight minutes. The distillates of fifty cc. each are received directly into the Nessler tubes marked at fifty cc., and the earlier portions may be Nesslerized while the distillation is progressing. Wanklyn advises to Nesslerize only the first distillate of fifty cc. free  $\text{NH}_3$  and add to this one-third. In waters containing enormous amounts of free ammonia, however, it is better either to distil the whole of the free ammonia into a flask in one portion, dilute, and take an aliquot part for the Nessler test, or to follow Dr. Smart's plan of Nesslerizing each successive distillate of fifty cc., previously diluting if necessary. Dr. Smart believes that the presence of urea in the water may be detected by peculiarities in the manner in which the free ammonia distils over in the successive portions. I have found that polluted waters containing enormous amounts of free ammonia such as to give a very deep color or a precipitate by the Nessler test in fifty cc. of the original sample appear to yield four-fifths or five-sixths of the whole amount in the first portion of fifty cc. instead of three-fourths, as stated by Wanklyn.

It is not advisable to put anything whatever, such as pieces of ignited pumice, tobacco pipe, etc., inside the retort to moderate the bumping, as suggested by some, there being too much danger of introducing thereby organic or ammoniacal impurity. The bumping can be controlled if necessary by lowering the flame and by tapping gently on the retort so as to produce a concentric wavy motion on the surface of the liquid, which will cause the steam to rise more regularly and in smaller bubbles. A Bunsen burner, giving a large flame, should be used. I prefer one having several jets, or a rose-burner, and place it quite close to the retort, without any intervening wire gauze.

All the apparatus used for water analysis should be set aside exclusively for this purpose. Thorough washing of each article immediately before use is very essential. To be thought "clean enough" is by no means sufficient. The



whole condensing apparatus set up ready for use, but disconnected from the retort, can be satisfactorily washed by flushing it thoroughly two or three times with clean hydrant water, closing the outlet by the finger or a clean cork until the condenser tube is full and then allowing the water to run out. This method of washing is much better than distilling some clean water through it as advised by some. The retort should be rinsed with a little concentrated sulphuric acid and washed thoroughly with clean tap water, and then immediately connected with the condenser.

The above form of apparatus may be considered by some to be cumbrous and to occupy too much space, yet I think it possesses certain real advantages which offset these objections.

The alkaline permanganate solution can readily be prepared so pure that one litre of it undiluted yields by distillation not more than 0.005 milligrammes  $\text{NH}_3$  and that no more ammonia develops in it afterward if properly protected. I have been using for more than ten years solutions prepared to this degree of purity, made up in quantities of one litre, which have kept well. The preparation is accomplished by using only very good distilled water for making up the solution, and by not permitting the sticks of caustic potash (ordinary white sticks) to come in contact with any kind of organic substance in handling and weighing it out. The mixed solution is diluted to one and one-half litres and distilled in a large retort, slowly, until about 900 cc. remain, and then 50 cc. are tested with Nessler solution. If pure, the solution is transferred when cold to a litre flask and made up to the mark with perfectly pure distilled water, made free from ammonia and organic matter by distillation with some solution of alkaline permanganate of potash. I found that if river water from a hydrant was used in making the solution it was very difficult to obtain a solution perfectly free from ammonia, even after very protracted slow boiling. If an impure solution is used, and a correction applied for the impurity it contains, it will be found that the results of analysis are thereby subjected to error. I strongly suspect that errors from that cause did occur in



some of my earliest analyses many years ago. Liability to error from this cause has been noticed also by others. Wanklyn, I think, never applied a correction for impurity, but always prepared the solution free from  $\text{NH}_3$ , if I understand rightly a certain discussion which took place in the early history of the process. Yet Dr. Tidy, in 1879, declared the preparation of a solution of alkaline permanganate, free from ammonia, to be impossible, and said that he always had to make a correction for the ammonia he found it to contain. This was one of the several objections that were strongly urged against the ammonia process of analysis. On the other hand, Prof. Mallet states, in his Report to the National Board of Health, 1882, that Dr. Smart in the investigations for that report made up his solution with good distilled water, and after the requisite boiling "it gave no coloration with Nessler reagent in fifty cc. distilled off from one-half litre."

Care should always be taken to rinse off the stoppered mouth of the bottle containing the permanganate solution both immediately before and after use, notwithstanding it may have been carefully protected from laboratory vapors, etc.

The rate at which both the free and albuminoid ammonia is evolved often affords valuable information as to the character of the organic matter contained in the water, and therefore the details of the separate Nessler testings should be preserved for reference. If, as sometimes happens, a peculiar odor is noticeable in the free ammonia distillate it may be presumed to be due to the volatile organic matter which has passed over with the free ammonia, but which will probably not produce a reaction with the Nessler test solution and be lost to the analysis. In this case a second distillation with alkaline permanganate added at first should be made with a fresh portion of the sample, thus obtaining the total ammonia, the free and albuminoid coming over together. If the amount of total ammonia is sensibly larger than the sum of the free and albuminoid obtained in the first distillation, the difference may be assumed to be ammonia from volatile organic matter.

From my own experience during about eight years, in

which I have very frequently repeated the distillation in the above manner, I am led to believe that the occurrence of a volatile organic substance in well waters is at least a comparatively rare circumstance. The test is, however, a useful check upon the accuracy of the analysis. The same proportions should be adhered to in the second as in the first distillation as regards the alkaline permanganate and the quantity of water operated upon.

Prof. Remsen found, however, in 1881 a striking instance of the presence of volatile organic matter, which failed to be estimated by the ordinary method of procedure in his investigations into the Water Supply of Baltimore. Soon afterwards, Mr. Chas. W. Marsh, then at Princeton, published a paper, in the *Amer. Chem. Journal*, giving a number of instances of similar nature in well and cistern waters which he had examined. The possibility of this occurrence should therefore be recognized and provision made for the estimation of the volatile organic substance.

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## ANALYSIS OF AN ARTESIAN WELL WATER.

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BY REUBEN HAINES.

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[Read at the stated meeting, held April 21, 1891.]

The well from which the water is derived, of which an analysis is here given, is situated in a closely built-up portion of Germantown. It was sunk in 1875 to the depth of 312 feet, mostly through soft rock which was probably a micaceous schist. I was informed that about half-way down a stratum was entered which carried large quantities of iron garnets, similar to those found in the surface rock and washings at Wayne Junction. At the bottom a cavity in the rock was apparently entered, for a portion of the drilling apparatus suddenly dropped, and, falling to one side, was lost and never recovered.

The rock was first struck at about eight feet from the surface of the ground. The water rose to a level of about thirteen feet below the surface, and the average flow by pumping has since been forty gallons per minute, without

lowering the water level materially. A continuous tube of plain wrought iron, about six inches in diameter, extends to within a few feet of the bottom.

The water was stated to have been originally of satisfactory character for steam purposes, although hard and unsuited to some technical uses.

In 1877, the writer made an analysis for its potable qualities with the following results:

	<i>Parts in 100,000.</i>
Free ammonia, . . . . .	0'0020
Albuminoid ammonia, . . . . .	0'0040
Chlorine, . . . . .	3'00
Total solid matter at 100° C., . . . .	30'00
Total hardness, . . . . .	16°·8 { parts CaCO <sub>3</sub> in 100,000 by soap test.
Nitrates, . . . . .	traces.
Sulphates, . . . . .	large amount.

The water has since that time deteriorated so much in quality that it is now seldom used, but pumped every few weeks only to keep it clear of sediment for use in case of fire. It has a strong corrosive action on the iron tube, so that after remaining in it for several weeks it becomes exceedingly thick and turbid with iron oxide. It was in this turbid condition when the writer took a sample for analysis a few months ago, and it was only partially cleared of sediment after pumping rapidly for more than an hour.

The following are the results of analysis for potability after the abundant iron oxide was allowed to settle out from which the water was decanted, but not filtered for the determination of the free and albuminoid ammonia. For the other parts of the analysis the water was filtered.

	<i>Parts in 100,000.</i>
Free ammonia, . . . . .	0'0590
Albuminoid ammonia, . . . . .	0'0060
Nitrogen in nitrates, . . . . .	0'77
Chlorine, . . . . .	3'10
Total solids dried at 120° C., . . . . .	54'50
Mineral matter, . . . . .	48'56
Organic and volatile matter by ignition, . . . . .	5'94
Temporary hardness by <i>Hehner's</i> method, . . . . .	10°·5 CaCO <sub>3</sub>
Permanent " " " " . . . . .	15°·0 "
Total " " " " . . . . .	25°·5 "

The following are the results of analysis of the mineral ingredients :

	<i>Parts in 100,000.</i>	<i>Grains per U. S. Gal.</i>
Na <sub>2</sub> O, . . . . .	7'44	4'341
(NH <sub>4</sub> ) <sub>2</sub> O, . . . . .	'09	'052
CaO, . . . . .	7'32	4'269
MgO, . . . . .	5'44	3'172
Fe <sub>2</sub> O <sub>3</sub> and Al <sub>2</sub> O <sub>3</sub> , . . . . .	'35	'204
SiO <sub>2</sub> , . . . . .	1'90	1'108
Cl, . . . . .	3'10	1'808
SO <sub>3</sub> , . . . . .	16'86	9'832
N <sub>2</sub> O <sub>5</sub> , . . . . .	2'97	1'732
CO <sub>2</sub> (calculated), . . . . .	4'65	2'715
Organic matter, H <sub>2</sub> O, etc., after deducting for loss of N <sub>2</sub> O <sub>5</sub> , CO <sub>2</sub> , etc., . . . . .	5'01	2'922

These constituents are probably combined in the following manner:

	<i>Parts in 100,000.</i>	<i>Grains per U. S. Gal.</i>
Ammonium nitrate, . . . . .	'180	'105
Sodium chloride, . . . . .	5'100	2'974
Sodium nitrate, . . . . .	4'533	2'643
Sodium sulphate, . . . . .	7'081	4'129
Calcium sulphate, . . . . .	17'777	10'367
Magnesium sulphate, . . . . .	3'624	2'113
Magnesium carbonate, . . . . .	8'887	5'183
Silica, ferric oxide, alumina, . . . . .	2'250	1'312
Organic matter, water of hydration, etc., .	5'010	2'922
	<hr/> 54'442	<hr/> 31'748

The Na<sub>2</sub>O was not estimated directly but calculated from the difference between the weighed solids converted into sulphates, and the CaO and MgO calculated as sulphate together with the SiO<sub>2</sub>, Fe<sub>2</sub>O<sub>3</sub> and Al<sub>2</sub>O<sub>3</sub>.

The test for hardness was made in the first analysis by the soap solution according to Clark, and in the recent analysis by Hehner's method which is much more accurate, especially in presence of magnesium salts.

The points of interest in regard to this well water are the increase of total solids and of hardness in a deep well water, and also the fact of its being so actively corrosive of iron in the cold.

SUPPLEMENT TO THE DETERMINATION OF THE  
HEAT OF EVAPORATION, ETC., OF LIQUID  
AMMONIA.\*

BY DR. HANS VON STROMBECK.

*[Presented at the stated meeting of the Chemical Section, held Jan. 19, 1891.]*

In vol. cxxx, No. 780 (December, 1890), of this journal, I gave the figures and authentic proofs for the heat of evaporation, etc., of liquid ammonia. The latter agreed well enough with one another for the purpose for which those tests were made, "to prove once more that about 500 and not 900 English thermal units were the true figures for the latent heat of liquid ammonia," but they do not agree well enough if considered from the scientific point of view. I, therefore, determined the latent heat of liquid ammonia by another series of twelve tests, the results of which I give in the following:

I made the tests and the calculations in the same manner as described in the number of this paper above-mentioned, but made the following alterations:

I *weighed* the water filled into the calorimeters instead of *measuring* it.

I placed each calorimeter in a box by itself, so that they were separated by a narrow stratum (about one-half inch) of external air. By this I intended to lessen the influence of the first calorimeter on the second one, but the figures afterwards showed that this intention was fulfilled to but a limited extent.

I took as much ammonia as with safety could be put in tank *IKL*, and I made the first calorimeter *ABCD* smaller, so as to have to take less water.

I placed two thermometers in the first calorimeter, one reading from 28 to 18°, the other from 20 to 11°.

I placed an agitator in the second calorimeter. Though this makes the handling of the apparatus still more compli-

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\* From the laboratory of the De La Vergne Refrigerating Machine Co.



cated, the agitator cannot be dispensed with, as only in this manner the true figures of the temperature prevailing in the second calorimeter can be obtained.

I determined the influence of the external surroundings in another manner from that of the former tests. In those, I followed the method used by Regnault, which requires the simultaneous reading of three thermometers at the end of every minute. In order to be made exact, these readings require three skilled observers, whom I had not at my disposal when I made the former tests, nor at present, and I attribute the discrepancy of the figures of my former tests mostly to my having been compelled to have the readings of two thermometers made by unskilled persons. I, therefore, determined the external influence this time in the following manner, which, theoretically, is not quite as exact as the one used by Regnault, but the errors of which are within the limit of the inevitable errors of observation, as I shall show by two examples, which follow.

#### DETERMINATION OF THE INFLUENCE OF THE EXTERNAL SURROUNDINGS ON THE TEMPERATURE OF THE CALORIMETERS.

I shall add to each item the figure obtained in the first test. As soon as, at the end of the test, the thermometer in the first calorimeter stops going down, the time  $m$  (42') elapsed since the commencement of the test and the temperatures indicated by the thermometer in the first calorimeter  $T_m$  ( $14^{\circ}79$ ), by the thermometer in the second calorimeter  $\tau_m$  ( $22^{\circ}75$ ), by the thermometer in the air  $\vartheta_m$  ( $21^{\circ}10$ ), are noted down. After  $n$  minutes (10') have elapsed, the temperatures indicated by the three thermometers  $T_n$  ( $14^{\circ}96$ ),  $\tau_n$  ( $22^{\circ}38$ ),  $\vartheta_n$  ( $21^{\circ}06$ ) are again noted down. From these figures the heating or cooling effect of the external surroundings on the first calorimeter  $\delta$  ( $-0^{\circ}221$ ) and on the second calorimeter  $\delta_1$  ( $0^{\circ}963$ ) can be calculated.

(1)  $\delta$ : The temperature of the first calorimeter is influenced by the temperature of the air and by that of the second calorimeter. But we may neglect the influence of the latter, its temperature being only little different from

the temperature of the air. During the  $n$  minutes (10') the first calorimeter was heated up by  $T_m - T_n$  degrees ( $-0^{\circ}17$ ), the difference between the average temperature of the first calorimeter

$$\frac{T_m + T_n}{2} (14^{\circ}88)$$

and the average temperature of the air

$$\frac{\vartheta_m + \vartheta_n}{2} (21^{\circ}08)$$

being  $-6^{\circ}20$ . During the  $m$  minutes (42') the difference between the average temperature of the first calorimeter

$$\frac{T + T_m}{2} (19^{\circ}595)$$

and the average temperature of the air

$$\frac{\vartheta + \vartheta_m}{2} (21^{\circ}52)$$

having been only  $-1^{\circ}925$ , we obtain:

$$\delta = \frac{(-0^{\circ}17) \cdot 42 \cdot (-1^{\circ}925)}{10 \cdot (-6^{\circ}20)} = -0^{\circ}221.$$

(2)  $\delta_1$ : The temperature of the second calorimeter is influenced by the temperature of the air and that of the first calorimeter. During the  $n$  minutes (10') the second calorimeter was cooled down by  $\tau_m - \tau_n$  degrees ( $0^{\circ}37$ ), its average temperature being

$$\frac{\tau_m + \tau_n}{2} (22^{\circ}565).$$

As the average temperature of the external surroundings is expressed by the average of the mean temperatures of the first calorimeter and of the air, *i. e.*, by

$$\frac{1}{2} \left[ \frac{T_m + T_n}{2} + \frac{\vartheta_m + \vartheta_n}{2} \right] = 17^{\circ}98,$$

the difference prevailing during the  $n$  minutes is  $22^{\circ}565 - 17^{\circ}98 = 4^{\circ}585$ . During the  $m$  minutes (42') the differ-

ence between the average temperature of the second calorimeter

$$\frac{\tau + \tau_m}{2} (23^{\circ}.40)$$

and the average temperature of the external surroundings

$$\frac{1}{2} \left[ \frac{T + T_m}{2} + \frac{\vartheta + \vartheta_m}{2} \right] = 20^{\circ}.558$$

having been only  $23^{\circ}.40 - 20^{\circ}.558 = 2^{\circ}.842$ , we obtain

$$\delta_1 = \frac{0.37.42 \cdot 2^{\circ}.842}{10.4585} = 0^{\circ}.963.$$

To make sure that the value obtained for  $\delta_1$  was the true expression for the external influence on the second calorimeter, I placed the first calorimeter *ABCD* with its appurtenances in the reverse position of that in the engraving on p. 473 (this journal, December, 1890); that is to say, so that the ammonia directly evaporated into the atmosphere, and operated exactly as I did, when determining the latent heat of liquid ammonia. All changes of temperature in the second calorimeter could therefore only be caused by the influence of the temperature of the first calorimeter and of the air. Making observations and calculations as above, we obtain one value for  $\delta_1 = \tau - \tau_m$  indicated by the thermometer, another value by calculation. If the letters have the same meaning as indicated, and the observations give:

	1.	2.
<i>m</i> , . . . . .	35'	30'
$\frac{T + T_m}{2}$ , . . . . .	18°'99	19°'30
$\frac{\vartheta + \vartheta_m}{2}$ , . . . . .	20°'03	20°'96
$\frac{T_m + T_n}{2}$ , . . . . .	13°'81	14°'20
$\frac{\vartheta_m + \vartheta_n}{2}$ , . . . . .	20°'08	20°'57
$\tau$ , . . . . .	22°'54	23°'82
$\tau_m$ , . . . . .	22°'23	23°'33

	1.	2.
$\frac{\tau + \tau_m}{2}$ . . . . .	22°·385	23°·575
$n$ , . . . . .	10'	10'
$\tau_n$ , . . . . .	22°·08	23°·06
$\tau_m - \tau_n$ , . . . . .	0°·15	0°·27
$\frac{\tau_m + \tau_n}{2}$ , . . . . .	22°·155	23°·195
<hr/>		
we have $\delta_1$ by observation, . . . . .	0°·31	0°·49
we have $\delta_1$ by calculation, . . . . .	0°·289	0°·470

If, in the following table,

$\mu$  = the weight of the first calorimeter and its appurtenances;

$\sigma$  = the specific heat of the metal (brass) of which it was made;

$\mu \sigma$  = its value in water;

$\mu_1 \sigma_1$  = the value in water of the two thermometers;

$\omega$  = the weight of the water filled into the first calorimeter;

$M = \omega + \mu \sigma + \mu_1 \sigma_1$ ;

$\phi T$  = the tension of the ammonia vapor at  $T^\circ$ ;

$\phi T_m$  = the tension of the ammonia vapor at  $T_m^\circ$ ;

$\phi \frac{T + T_m}{2}$  = the tension of the ammonia vapor at  $\frac{T + T_m}{2}$ ;

$m$  = the number of minutes which the test lasted.

$T$  = the temperature of the first calorimeter at the commencement of the test;

$T_m$  = the temperature of the first calorimeter at the end of the test;

$\frac{T + T_m}{2}$  = the average of both temperatures;

$\vartheta$  = the temperature of the air at the commencement of the test;

$\vartheta_m$  = the temperature of the air at the end of the test ;

$\frac{\vartheta + \vartheta_m}{2}$  = the average of both temperatures ;

$n$  = the number of minutes during which observations were made for the determination of  $\delta_1$  and  $\delta_2$ , respectively ;

$T_n$  = the temperature of the first calorimeter at the end of the  $n$  minutes ;

$T_m - T_n$  = the fraction of a degree by which the temperature of the first calorimeter changed during the  $n$  minutes ;

$\frac{T_m + T_n}{2}$  = the average of both temperatures ;

$\vartheta_n$  = the temperature of the air at the end of the  $n$  minutes ;

$\frac{\vartheta_m + \vartheta_n}{2}$  = the average of both temperatures ;

$\delta$  = the fraction of a degree which is to be added to or subtracted from  $T_m$  so as to obtain the temperature ;

$T_m + \delta$  which the first calorimeter would have had if no external influence had taken place ;

$T - (T_m + \delta)$  = the corrected number of degrees by which the first calorimeter was cooled down ;

$M [T - (T_m + \delta)]$  = the loss of heat undergone by the first calorimeter ;

$\mu_2$  = the weight of the second calorimeter and its appurtenances ;

$\mu_2 \sigma$  = its value in water ;

$\mu_3 \sigma_3$  = the value in water of the thermometer ;

$\omega_1$  = the weight of water filled into the second calorimeter ;

$M_1 = \omega_1 + \mu_2 \sigma + \mu_3 \sigma_3$  ;

$\tau$  = the temperature of the second calorimeter at the commencement of the test ;



$\tau_m$  = the temperature of the second calorimeter at the end of the test;

$\frac{\tau + \tau_m}{2}$  = the average of both temperatures;

$\tau_n$  = the temperature of the second calorimeter at the end of the  $n$  minutes;

$\tau_m - \tau_n$  = the fraction of a degree by which the temperature of the second calorimeter changed during the  $n$  minutes;

$\frac{\tau_m + \tau_n}{2}$  = the average of both temperatures;

$\delta_1$  = the number of degrees which is to be added to  $\tau_m$  so as to obtain the temperature:

$\tau_m + \delta_1$  which the second calorimeter would have had if no external influence had taken place;

$\tau - (\tau_m + \delta_1)$  = the corrected number of degrees by which the second calorimeter was cooled down;

$M_1 [\tau - (\tau_m + \delta)]$  = the loss of heat undergone by the second calorimeter;

$\frac{T + T_m}{2} - \frac{\tau + \tau_m}{2}$  = the difference between the average temperatures of both calorimeters;

and if the other letters have the same meaning as in my first article on this matter, we obtain from the tests and by calculation the figures given in the accompanying table:

1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	AVERAGE
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$\delta_1$	$\delta_2$	21°05	21°15	21°25	21°35	21°45	21°55	22°05	22°15	22°25	22°35	22°45	22°55	23°05	23°15	23°25	23°35	23°45	23°55	24°05	24°15	24°25	24°35	24°45	24°55	25°05	25°15	25°25	25°35	25°45	25°55	26°05	26°15	26°25	26°35	26°45	26°55	27°05	27°15	27°25	27°35	27°45	27°55	28°05	28°15	28°25	28°35	28°45	28°55	29°05	29°15	29°25	29°35	29°45	29°55	30°05	30°15	30°25	30°35	30°45	30°55	31°05	31°15	31°25	31°35	31°45	31°55	32°05	32°15	32°25	32°35	32°45	32°55	33°05	33°15	33°25	33°35	33°45	33°55	34°05	34°15	34°25	34°35	34°45	34°55	35°05	35°15	35°25	35°35	35°45	35°55	36°05	36°15	36°25	36°35	36°45	36°55	37°05	37°15	37°25	37°35	37°45	37°55	38°05	38°15	38°25	38°35	38°45	38°55	39°05	39°15	39°25	39°35	39°45	39°55	40°05	40°15	40°25	40°35	40°45	40°55	41°05	41°15	41°25	41°35	41°45	41°55	42°05	42°15	42°25	42°35	42°45	42°55	43°05	43°15	43°25	43°35	43°45	43°55	44°05	44°15	44°25	44°35	44°45	44°55	45°05	45°15	45°25	45°35	45°45	45°55	46°05	46°15	46°25	46°35	46°45	46°55	47°05	47°15	47°25	47°35	47°45	47°55	48°05	48°15	48°25	48°35	48°45	48°55	49°05	49°15	49°25	49°35	49°45	49°55	50°05	50°15	50°25	50°35	50°45	50°55	51°05	51°15	51°25	51°35	51°45	51°55	52°05	52°15	52°25	52°35	52°45	52°55	53°05	53°15	53°25	53°35	53°45	53°55	54°05	54°15	54°25	54°35	54°45	54°55	55°05	55°15	55°25	55°35	55°45	55°55	56°05	56°15	56°25	56°35	56°45	56°55	57°05	57°15	57°25	57°35	57°45	57°55	58°05	58°15	58°25	58°35	58°45	58°55	59°05	59°15	59°25	59°35	59°45	59°55	60°05	60°15	60°25	60°35	60°45	60°55	61°05	61°15	61°25	61°35	61°45	61°55	62°05	62°15	62°25	62°35	62°45	62°55	63°05	63°15	63°25	63°35	63°45	63°55	64°05	64°15	64°25	64°35	64°45	64°55	65°05	65°15	65°25	65°35	65°45	65°55	66°05	66°15	66°25	66°35	66°45	66°55	67°05	67°15	67°25	67°35	67°45	67°55	68°05	68°15	68°25	68°35	68°45	68°55	69°05	69°15	69°25	69°35	69°45	69°55	70°05	70°15	70°25	70°35	70°45	70°55	71°05	71°15	71°25	71°35	71°45	71°55	72°05	72°15	72°25	72°35	72°45	72°55	73°05	73°15	73°25	73°35	73°45	73°55	74°05	74°15	74°25	74°35	74°45	74°55	75°05	75°15	75°25	75°35	75°45	75°55	76°05	76°15	76°25	76°35	76°45	76°55	77°05	77°15	77°25	77°35	77°45	77°55	78°05	78°15	78°25	78°35	78°45	78°55	79°05	79°15	79°25	79°35	79°45	79°55	80°05	80°15	80°25	80°35	80°45	80°55	81°05	81°15	81°25	81°35	81°45	81°55	82°05	82°15	82°25	82°35	82°45	82°55	83°05	83°15	83°25	83°35	83°45	83°55	84°05	84°15	84°25	84°35	84°45	84°55	85°05	85°15	85°25	85°35	85°45	85°55	86°05	86°15	86°25	86°35	86°45	86°55	87°05	87°15	87°25	87°35	87°45	87°55	88°05	88°15	88°25	88°35	88°45	88°55	89°05	89°15	89°25	89°35	89°45	89°55	90°05	90°15	90°25	90°35	90°45	90°55	91°05	91°15	91°25	91°35	91°45	91°55	92°05	92°15	92°25	92°35	92°45	92°55	93°05	93°15	93°25	93°35	93°45	93°55	94°05	94°15	94°25	94°35	94°45	94°55	95°05	95°15	95°25	95°35	95°45	95°55	96°05	96°15	96°25	96°35	96°45	96°55	97°05	97°15	97°25	97°35	97°45	97°55	98°05	98°15	98°25	98°35	98°45	98°55	99°05	99°15	99°25	99°35	99°45	99°55	100°05	100°15	100°25	100°35	100°45	100°55	101°05	101°15	101°25	101°35	101°45	101°55	102°05	102°15	102°25	102°35	102°45	102°55	103°05	103°15	103°25	103°35	103°45	103°55	104°05	104°15	104°25	104°35	104°45	104°55	105°05	105°15	105°25	105°35	105°45	105°55	106°05	106°15	106°25	106°35	106°45	106°55	107°05	107°15	107°25	107°35	107°45	107°55	108°05	108°15	108°25	108°35	108°45	108°55	109°05	109°15	109°25	109°35	109°45	109°55	110°05	110°15	110°25	110°35	110°45	110°55	111°05	111°15	111°25	111°35	111°45	111°55	112°05	112°15	112°25	112°35	112°45	112°55	113°05	113°15	113°25	113°35	113°45	113°55	114°05	114°15	114°25	114°35	114°45	114°55	115°05	115°15	115°25	115°35	115°45	115°55	116°05	116°15	116°25	116°35	116°45	116°55	117°05	117°15	117°25	117°35	117°45	117°55	118°05	118°15	118°25	118°35	118°45	118°55	119°05	119°15	119°25	119°35	119°45	119°55	120°05	120°15	120°25	120°35	120°45	120°55	121°05	121°15	121°25	121°35	121°45	121°55	122°05	122°15	122°25	122°35	122°45	122°55	123°05	123°15	123°25	123°35	123°45	123°55	124°05	124°15	124°25	124°35	124°45	124°55	125°05	125°15	125°25	125°35	125°45	125°55	126°05	126°15	126°25	126°35	126°45	126°55	127°05	127°15	127°25	127°35	127°45	127°55	128°05	128°15	128°25	128°35	128°45	128°55	129°05	129°15	129°25	129°35	129°45	129°55	130°05	130°15	130°25	130°35	130°45	130°55	131°05	131°15	131°25	131°35	131°45	131°55	132°05	132°15	132°25	132°35	132°45	132°55	133°05	133°15	133°25	133°35	133°45	133°55	134°05	134°15	134°25	134°35	134°45	134°55	135°05	135°15	135°25	135°35	135°45	135°55	136°05	136°15	136°25	136°35	136°45	136°55	137°05	137°15	137°25	137°35	137°45	137°55	138°05	138°15	138°25	138°35	138°45	138°55	139°05	139°15	139°25	139°35	139°45	139°55	140°05	140°15	140°25	140°35	140°45	140°55	141°05	141°15	141°25	141°35	141°45	141°55	142°05	142°15	142°25	142°35	142°45	142°55	143°05	143°15	143°25	143°35	143°45	143°55	144°05	144°15	144°25	144°35	144°45	144°55	145°05	145°15	145°25	145°35	145°45	145°55	146°05	146°15	146°25	146°35	146°45	146°55	147°05	147°15	147°25	147°35	147°45	147°55	148°05	148°15	148°25	148°35	148°45	148°55	149°05	149°15	149°25	149°35	149°45	149°55	150°05	150°15	150°25	150°35	150°45	150°55	151°05	151°15	151°25	151°35	151°45	151°55	152°05	152°15	152°25	152°35	152°45	152°55	153°05	153°15	153°25	153°35	153°45	153°55	154°05	154°15	154°25	154°35	154°45	154°55	155°05	155°15	155°25	155°35	155°45	155°55	156°05	156°15	156°25	156°35	156°45	156°55	157°05	157°15	157°25	157°35	157°45	157°55	158°05	158°15	158°25	158°35	158°45	158°55	159°05	159°15	159°25	159°35	159°45	159°55	160°05	160°15	160°25	160°35	160°45	160°55	161°05	161°15	161°25	161°35	161°45	161°55	162°05	162°15	162°25	162°35	162°45	162°55	163°05	163°15	163°25	163°35	163°45	163°55	164°05	164°15	164°25	164°35	164°45	164°55	165°05	165°15	165°25	165°35	165°45	165°55	166°05	166°15	166°25	166°35	166°45	166°55	167°05	167°15	167°25	167°35	167°45	167°55	168°05	168°15	168°25	168°35	168°45	168°55	169°05	169°15	169°25	169°35	169°45	169°55	170°05	170°15	170°25	170°35	170°45	170°55	171°05	171°15	171°25	171°35	171°45	171°55	172°05	172°15	172°25	172°35	172°45	172°55	173°05	173°15	173°25	173°35	173°45	173°55	174°05	174°15	174°25	174°35	174°45	174°55	175°05	175°15	175°25	175°35	175°45	175°55	176°05	176°15	176°25	176°35	176°45	176°55	177°05	177°15	177°25	177°35	177°45	177°55	178°05	178°15	178°25	178°35	178°45	178°55	179°05	179°15	179°25	179°35	179°45	179°55	180°05	180°15	180°25	180°35	180°45	180°55	181°05	181°15	181°25	181°35	181°45	181°55	182°05	182°15	182°25	182°35	182°45	182°55	183°05	183°15	183°25	183°35	183°45	183°55	184°05	184°15	184°25	184°35	184°45	184°55	185°05	185°15	185°25	185°35	185°45	185°55	186°05	186°15	186°25	186°35	186°45	186°55	187°05	187°15	187°25	187°35	187°45	187°55	188°05	188°15	188°25	188°35	188°45	188°55	189°05	189°15	189°25	189°35	189°45	189°55	190°05	190°15	190°25	190°35	190°45	190°55	191°05	191°15	191°25	191°35	191°45	191°55	192°05	192°15	192°25	192°35	192°45	192°55	193°05	193°15	193°25	193°35	193°45	193°55	194°05	194°15	194°25	194°35	194°45	194°55	195°05	195°15	195°25	195°35	195°45	195°55	196°05	196°15	196°25	196°35	196°45	196°55	197°05	197°15	197°25	197°35	197°45	197°55	198°05	198°15	198°25	198°35	198°45	198°55	199°05	199°15	199°25	199°35	199°45	199°55	200°05	200°15	200°25	200°35	200°45	200°55	201°05	201°15	201°25	201°35	201°45	201°55	202°05	202°15	202°25	202°35	202°45	202°55	203°05	203°15	203°25	203°35	203°45	203°55	204°05	204°15	204°25	204°35	204°45	204°55	205°05	205°15	205°25	205°35	205°45	205°55	206°05	206°15	206°25	206°35	206°45	206°55	207°05	207°15	207°25	207°35	207°45	207°55	208°05	208°15	208°25	208°35	208°45	208°55	209°05	209°15	209°25	209°35	209°45	209°55	210°05	210°15	210°25	210°35	210°45	210°55	211°05	211°15	211°25	211°35	211°45	211°55	212°05	212°15	212°25	212°35	212°45	212°55	213°05	213°15	213°25	213°35	213°45	213°55	214°05	214°15	214°25	214°35	214°45	214°55	215°05	215°15	21
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## ON THE HEAT OF FORMATION OF FLUORIDES.

BY JOS. W. RICHARDS.

[Read at the meeting of the Chemical Section, held April 21, 1891.]

Until the heat of formation of some fluorine compound from its elements was known, it was manifestly impossible to determine the heat of formation of the fluorides. On the other hand, as soon as the heat of formation of any fluorine compound had been determined, that of any other fluorine compound could be deduced from this by the calorimetric measurement of a double reaction, in which the first compound took part and the second was produced. Many data of this latter kind had been accumulated before the fundamental datum of the first kind was obtained, for the simple reason that free fluorine is required in order to determine primarily the heat of formation of a fluorine compound, while the double reactions referred to above only investigate the thermal differences between various fluorine compounds, and so do not require free fluorine.

Guntz, in 1883,\* published some data which he miscalled "Heats of Formation of Fluorides." Under this caption he gives the following figures:

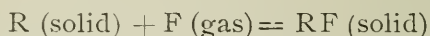
Potassium fluoride,	38.2 cal.
Sodium "	39.9 "
Barium "	35.7 "
Strontium "	35.9 "
Calcium "	33.3 "
Ammonium "	37.3 "

The writer knows of at least one well-educated chemist who was misled by the manner in which Guntz gives these figures, into quoting 39.9 calories as the real heat of formation of sodium fluoride. However, on examining Guntz's paper in detail, we find that he neutralized the metallic hydrates in solution by aqueous hydrofluoric acid, and having determined the collateral data—the heat of solution

\* *Comptes Rendus*, **97**, 1483, 1558; **98**, 816.

of the hydrates, fluorides and hydrofluoric acid—he calculated the heat which would be developed if gaseous hydrofluoric acid acted on the solid hydrates, forming solid fluorides and solid water. These numbers he called the heats of formation of these fluorides, and they are the numbers given above.

In 1884,\* Guntz published a memoir on "Thermal Researches on the Combinations of Fluorine with Metals," in which he gives the heats of neutralization of the hydrates of potassium, sodium, ammonium, calcium, strontium, barium, magnesium, lead and silver, in aqueous solution by hydrofluoric acid. He also gives the heats of solution in water of the fluorides of potassium, sodium, ammonium, silver and antimony (the others being insoluble), and of gaseous and liquid hydrofluoric acid. With these data, it was possible for Guntz to calculate the absolute heat of formation of the above fluorides, expressing them as a certain excess above the heat of formation of gaseous hydrofluoric acid, which was an unknown quantity in all the equations. His results were as follows (calling the unknown quantity, [H, F], X):



Potassium, . . . . .	KF	71.7 + X cal.
Sodium, . . . . .	NaF	71.8 + X "
Calcium, . . . . .	$\frac{1}{2}$ (CaF <sup>2</sup> )	74.3 + X "
Strontium, . . . . .	$\frac{1}{2}$ (Sr F <sup>2</sup> )	71.0 + X "
Magnesium, . . . . .	$\frac{1}{2}$ (MgF <sup>2</sup> )	67.2 + X "
Lead, . . . . .	$\frac{1}{2}$ (Pb F <sup>2</sup> )	15.1 + X "
Silver, . . . . .	Ag F	— 11.9 + X "
Antimony, . . . . .	$\frac{1}{3}$ (Sb F <sup>3</sup> )	8.5 + X "
Silicon, . . . . .	$\frac{1}{4}$ (Si F <sup>4</sup> )	29.9 + X "

Guntz even went so far as to make a guess as to what the value X might be, with a somewhat remarkable result. He argued that since chlorine gas does not decompose anhydrous potassium fluoride, the heat of formation of the latter must be superior to that of potassium chloride, which is 105.0 calories. Further, if the difference between these two numbers is assumed to be similar to that between the

\* *Annales de Chimie et de Physique*, [vi], 3, 5.



heats of formation of potassium chloride and bromide, we may add this difference to 105.0 calories, and call the result the heat of formation of potassium fluoride, which can then be placed equal to  $71.7 + X$ , and  $X$  evaluated. Guntz here makes two serious and almost incomprehensible blunders. There is absolutely no warrant for the above assumption, and it could be true only by the merest accident. Further, Guntz assumes the difference spoken of as 4.6 calories. Where this number comes from is incomprehensible, for the difference derived from Thomsen's data is 10.3, and the source of the number 4.6 is veiled in mystery. However, using this number, the value of  $X$  found was 37.5 calories. That this number was the true one could be possible only by the greatest chance, as it rested on a false assumption and the use of a wrong datum, yet by a strange fortuity this number is only 0.1 calorie from the direct determination made five years later. Guntz deserves much credit for the work he did, but he can hardly claim any renown from this prediction.

Two years later,\* Moissan isolated fluorine, and found that it decomposed solid potassium chloride in the cold, disengaging chlorine, thus making it probable in a high degree that the heat of formation of potassium fluoride was greater than that of the chloride, that therefore  $71.7 + X$  calories was greater than 105.6, and consequently  $X$  greater than 33.9 calories.

In 1889,† Berthelot and Moissan succeeded in determining, experimentally, the heat of formation of hydrofluoric acid. They found:



This was the key which unlocked the question of the heats of formation of the fluorides, for since Guntz had determined the heat of solution of hydrofluoric acid gas in water as 11.8 calories, the heat of formation of the fluorides in aqueous solution could then be calculated from the heats

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\* *Comptes Rendus*, **102**, 1543.

† *Idem*, **109**, 209.

of neutralization of the metallic hydrates by aqueous hydrofluoric acid.

In 1889 and 1890,\* E. Petersen, of Copenhagen, did good service in the line we are considering, by determining the heats of neutralization of most of the metallic hydrates by hydrofluoric acid.

The following table gives the data obtained by Guntz and Petersen :

Q.	HEAT EVOLVED BY THE REACTION Q + 2 HF aq.	
	Guntz.	Petersen.
2LiOHaq, . . . . .	—	32.80 cal.
2KOHaq, . . . . .	32.30 cal.	32.20 "
2NaOHaq, . . . . .	32.60 "	32.54 "
2TlOHaq, . . . . .	— "	32.90 "
2NH <sup>+</sup> OHaq, . . . . .	30.40 "	—
Ba (OH) <sup>2</sup> aq, . . . . .	*34.80 "	32.30 "
Sr(OH) <sup>2</sup> aq, . . . . .	*35.80 "	35.47 "
Ca(OH) <sup>2</sup> aq, . . . . .	*37.26 "	36.31 "
Mg(OH) <sup>2</sup> , . . . . .	*30.40 "	30.12 "
Be(OH) <sup>2</sup> , . . . . .	—	19.68 "
Mn(OH) <sup>2</sup> , . . . . .	—	23.10 "
Zn(OH) <sup>2</sup> , . . . . .	—	25.10 "
Cd(OH) <sup>2</sup> , . . . . .	—	25.56 "
Fe(OH) <sup>2</sup> , . . . . .	—	26.53 "
Co(OH) <sup>2</sup> , . . . . .	—	26.49 "
Ni(OH) <sup>2</sup> , . . . . .	—	27.67 "
Cu(OH) <sup>2</sup> , . . . . .	—	20.17 "
PbO, . . . . .	*22.2 "	—
Ag <sup>2</sup> O, . . . . .	14.6 "	—
1/3 Fe <sup>2</sup> (OH) <sup>6</sup> , . . . . .	—	15.83 "
1/3 Al <sup>2</sup> (OH) <sup>6</sup> , . . . . .	—	23.42 "
1/3 Cr <sup>2</sup> (OH) <sup>6</sup> , . . . . .	—	16.78 "
1/3 Mn <sup>2</sup> (OH) <sup>6</sup> , . . . . .	—	17.21 "
1/3 V <sup>2</sup> (OH) <sup>6</sup> , . . . . .	—	17.41 "
1/4 SiO <sup>2</sup> aq, . . . . .	†11.3 "	—
1/4 B <sup>2</sup> O <sup>3</sup> aq, . . . . .	‡9.2 "	—
1/3 Sb <sup>2</sup> O <sup>3</sup> aq, . . . . .	10.1 "	—

\* Resulting fluoride precipitated.

† An excess of acid, 8 HF, had to be used, forming a fluorhydrate.

‡ Determined by Thomsen.

§ Thomsen found the same value.

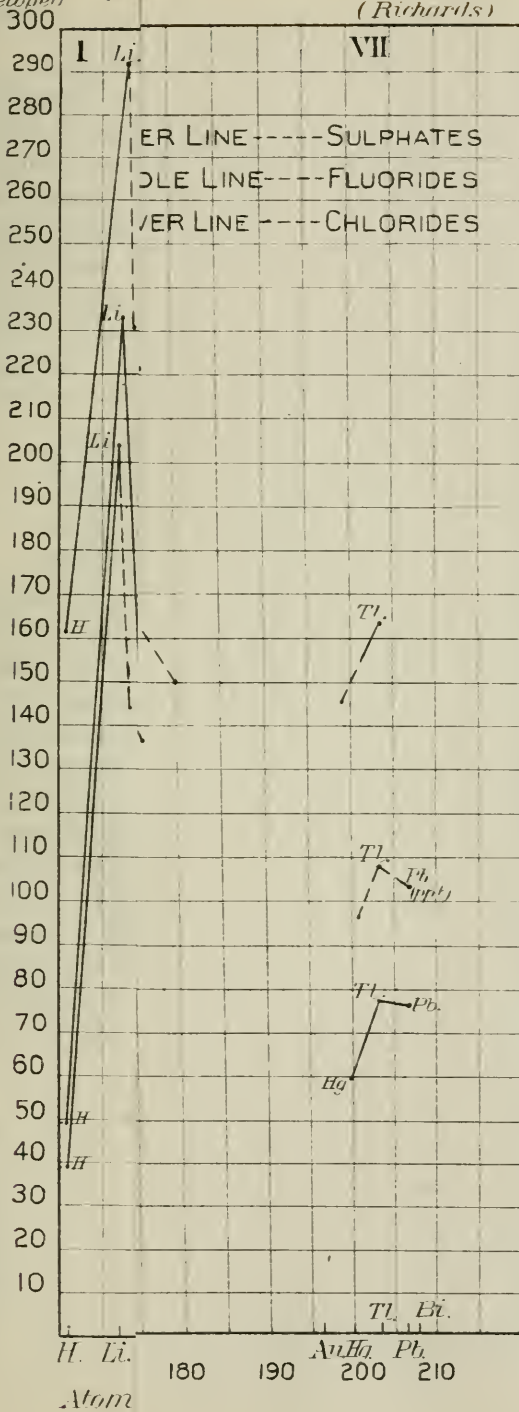
In order to use these data correctly, we must notice the

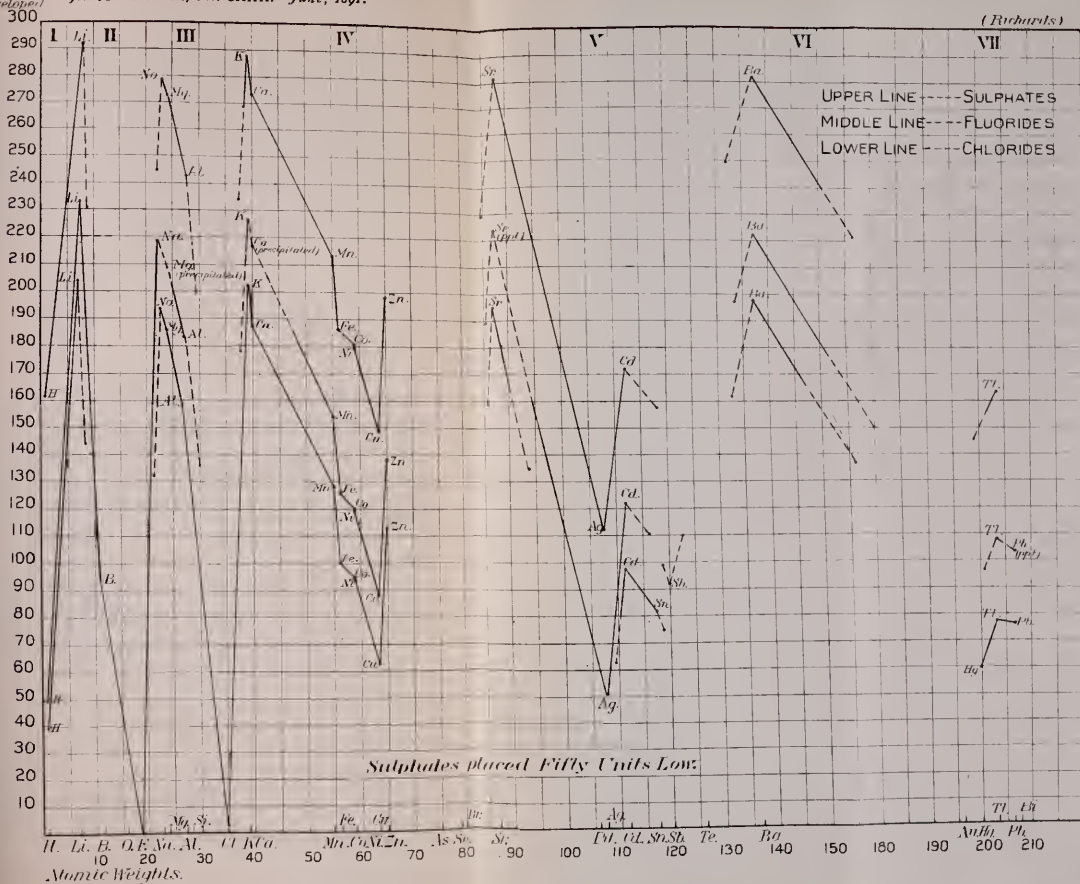
\* *Zeitschrift für physikalische Chemie*, 4, 385; 5, 258.

Heat  
Development

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fact of the insolubility of some of the fluorides. Petersen found that by working with very dilute solutions all the fluorides he tried were soluble excepting those of strontium, calcium, magnesium and lead. Guntz, for instance, found barium fluoride nearly insoluble, and gave his value as a close approximation to that for precipitated barium fluoride, while Petersen dissolved it in a large excess of water and obtained a result 2.50 calories lower, showing the heat of solution of barium fluoride to be close to or a little less than 2.50 calories. The heats of solution of the following fluorides have been determined:

Potassium fluoride,	+ 3.6 cal.	} Per formula weight of salt dissolved.
Sodium "	— 0.6 "	
Ammonium "	— 1.5 "	
Silver "	+ 3.4 "	
Antimony "	— 4.0 "	

For the above fluorides, then, we can calculate their heats of formation anhydrous as well as in aqueous solution. We can do this, approximately, for barium fluoride also. For the insoluble fluorides their heat of formation in the solid state only can be found. For all the other fluorides we can calculate only their heat of formation in aqueous solution, and this only when we know the heat of formation of the metallic hydrate to start with. This latter factor is not known with respect to beryllium, chromic, manganic and vanadic hydrates.

Starting with the data thus presented, the writer has calculated the heats of formation of those fluorides for which there is sufficient data, using generally the heats of formation of the hydrates as given by Thomsen.

#### HEATS OF FORMATION OF FLUORIDES.

<i>M.</i>	<i>Products Solid.</i> [ <i>MF</i> <sup>2</sup> ]	<i>Heat of Solution.</i> [ <i>MF</i> <sup>2</sup> , aq]	<i>Products Dissolved.</i> [ <i>MF</i> <sup>2</sup> , aq]
Li <sup>2</sup> , . . . . .	—	—	233.76
K <sup>2</sup> , . . . . .	220.00	+ 7.20	227.20
Na <sup>2</sup> , . . . . .	219.44	— 1.20	218.24
Tl <sup>2</sup> , . . . . .	—	—	108.81
[NH <sub>4</sub> ] <sup>2</sup> , . . . . .	202.50	— 3.00	199.50
Ba, . . . . .	224.00	— 2.50	221.50
Sr, . . . . .	224.02	—	—



## HEATS OF FORMATION OF FLUORIDES.—(Continued.)

<i>M.</i>	<i>Products Solid.</i> [ <i>MF</i> <sup>2</sup> ]	<i>Heat of Solution.</i> [ <i>MF</i> <sup>2</sup> , aq]	<i>Products Dissolved.</i> [ <i>MF</i> <sup>2</sup> , aq]
Ca, . . . . .	{ 216·90* 216·00†	—	—
Mg, . . . . .	209·50	—	—
Mn, . . . . .	—	—	153·31
Zn, . . . . .	—	—	138·22
Cd, . . . . .	—	—	121·72
Fe, . . . . .	—	—	125·22
Co, . . . . .	—	—	120·34
Ni, . . . . .	—	—	118·98
Cu, . . . . .	—	—	88·16
Pb, . . . . .	{ 100·71* 102·50*		
Ag <sup>2</sup> , . . . . .	44·14	+ 6·8	50·94
$\frac{1}{3}$ F <sup>2</sup> , . . . . .	—	—	109·96
$\frac{1}{3}$ Al <sup>2</sup> , . . . . .	—	—	183·48
$\frac{1}{2}$ Si, . . . . .	137·96	—	—
$\frac{1}{3}$ Sb <sup>2</sup> , . . . . .	92·79	— 1·67	91·12
$\frac{1}{3}$ B <sup>2</sup> , . . . . .	—	—	146·23

\* Based on Guntz's experiment.

† Based on Petersen's experiment.

In the large diagram accompanying this paper, I have used the atomic weights of the elements as abscissas, and the heat of formation of their fluorides in aqueous solution as ordinates. The resultant connecting line, though fragmentary, shows very clearly the periodic variation of these heats of formation with the increase of atomic weight, dividing the elements into seven families corresponding to the seven families of the well-known classification of the elements by the Periodic Law. Mr. A. P. Laurie, in 1883,\* arranged the chlorides in this way, and I have placed the chloride line on the diagram as well as the line of the sulphates (the latter brought down fifty units for convenience in drawing the chart). An inspection of the diagram shows a very near approach to parallelism in these three lines, the variations being hardly perceptible on the chart; but, if we tabulate the numerical differences between the heats of formation of these three classes of compounds, we will discern the irregularities more easily. We will only com-

\* *Philosophical Magazine*, 15, 41.

pare those elements for which we know the heats of formation of the three salts, all in aqueous solution with the exception of those marked with a star.

<i>M.</i>	CHLORIDES FROM FLUORIDES.		FLUORIDES FROM SULPHATES	
	— $\frac{[MF^2, aq]}{[MCl^2, aq]}$	Deviation from mean difference.	— $\frac{[MSO^4, aq]}{[MF^2, aq]}$	Deviation from mean difference.
Li <sup>2</sup> , . . . . .	29'36	+ 4'17	106'56	— 3'92
K <sup>2</sup> , . . . . .	24'99	— 0'20	111'22	+ 0'74
Na <sup>2</sup> , . . . . .	25'22	+ 0'03	111'21	+ 0'73
Ba, . . . . .	24'69	— 0'50	110'98	+ 0'50
Sr, . . . . .	*28'03	+ 2'84	*106'90	— 3'58
Ca, . . . . .	*28'77	+ 3'58	*106'55	— 3'92
Mg, . . . . .	*22'57	— 2'62	*113'16	— 2'68
$\frac{1}{3}A^2$ , . . . . .	24'93	— 0'26	109'57	— 0'91
$\frac{1}{3}F^2$ , . . . . .	24'81	— 0'38	—	—
Mn, . . . . .	25'31	+ 0'12	110'34	— 0'14
Zn, . . . . .	25'38	+ 0'19	110'27	— 0'21
Cd, . . . . .	25'47	+ 0'28	110'56	+ 0'08
Fe, . . . . .	25'27	+ 0'08	110'38	— 0'10
Co, . . . . .	25'52	+ 0'33	110'13	— 0'35
Ni, . . . . .	25'28	— 0'09	110'37	— 0'11
Cu, . . . . .	25'35	+ 0'16	110'30	— 0'18
Mean value, counting the insoluble fluorides and lithium, }	25'19	—	110'48	—

An inspection of this table shows us at once the irregularity in the case of the insoluble fluorides. Magnesium fluoride is 2'62 calories too low in one case and 2'68 in the other, or a mean of 2'65. It is therefore probable that when this fluoride precipitates from a solution it absorbs about 2'65 calories, and that if it could be retained in a very dilute solution, its heat of formation would be 2'65 calories greater. Similarly, the heat of formation of calcium fluoride in aqueous solution might be about 3'75 calories, and of strontium fluoride 3'25 calories less than when the fluorides are precipitated. But what are we to say of lithium, that pattern of good behavior in all its other thermal relations? Here is certainly an anomaly, whose explanation, I think, lies in the fact that lithium fluoride is the most nearly insoluble salt of lithium, and Petersen has either omitted to say that there was a precipitate in this case, or he has given us a wrong value for the reaction. We have very strong grounds for concluding that the true heat of forma-

tion of lithium fluoride in aqueous solution is about four calories less than the value which is deduced from Petersen's experiments on neutralization. The variations in the case of the other fluorides in the above table are so small that we may conclude that we know their true heat of formation within the limits of experimental error.

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## THE DIAZOTYPE PROCESS OF PHOTOGRAPHIC DYEING AND PRINTING.

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BY JOHN CARBUTT.

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[Communicated to the Chemical Section, April 21, 1891.]

The subject I have the pleasure to bring before the Section this evening is the "Diazotype Process of Photographic Dyeing and Printing," the discovery of Mr. Arthur G. Green, of London. It is a new method of producing designs in fast colors on cotton, linen, silk, paper, etc., either from hand-drawn designs on a transparent or semi-transparent medium, or by use of a photographic transparency.

It was first made public at a meeting of the British Association for the Advancement of Science, held at Leeds last September.

It is distinctly a positive process; that is, it yields a positive from a positive. The operations are five in number, viz: dyeing, sensitizing, printing, developing and washing, corresponding very closely in number of operations required in preparing and printing on paper by the direct silver process of photographic printing. Primuline is the base of the new "diazotype," and is already in extensive use for dyeing calico in various tints.

*Dyeing.*—The first operation, then, is to dye our material with "primuline." To do this take 100 grains of "primuline," add to it twenty ounces of distilled water brought to a boil in a flask over a Bunsen burner; when dissolved, pour into a porcelain dish, and immerse the fabric, cotton cloth (the cotton must be free from dressing), or silk, and as soon as the pieces seem thoroughly impregnated with the dye,

remove and rinse in plain water, and squeeze as dry as possible, or place between sheets of blotting paper. The fabric so prepared has a primrose hue, and so far is insensitive to light.

*Sensitizing.*—The second operation, or sensitizing, is accomplished by immersing the dyed fabric in a solution formed of sodium nitrate sixty grains, cold water thirty-two ounces, oxalic acid 100 grains, previously dissolved in two ounces of water. Each piece is immersed separately and soaked for two or three minutes, it is then rinsed in water and dried. The color of the fabric should be of a reddish-yellow hue, and is sensitive to light; the operation should be performed in a weak light, and the pieces dried in the dark.

*Printing.*—Place in a printing frame a glass transparency, and place over it a diazotized fabric in contact, and expose to daylight ten to twenty minutes, or sunlight four to seven minutes. The greater the contrast in the transparency, the greater will be the contrast in the finished print, the action of daylight being to decompose the azo derivative, and so prevent reaction with the “developers,” so-called, to be afterwards applied; but if we take a piece of the sensitive material that has not been exposed to light, and apply one or more of the developers, or, as a dyer would call it, a “mordant,” we at once develop the full tint or color by the action of that particular solution used. This I will now illustrate.

*Developing.*—I have here a piece of porcelain coated with a solution of gelatine and primuliné, and sensitized in the same bath as the cotton cloth. I now paint over one-fifth of its surface, consecutively, with each of the following five solutions or developers:

No. 1, as you see, develops a red color.

No. 2 produces a yellow.

No. 3 produces an orange.

No. 4 produces a purple.

No. 5 produces a dark brown or black.

It only remains to well rinse this in water, when the colors will be found fixed and permanent. This demonstra-

tion will illustrate the possibility of producing the several colors on one and the same picture, as follows: Mix a portion of each developer with a thin starch paste, and, with a camel's-hair pencil, paint over the parts wanted of a particular color, with the particular solution intended to supply that color, of course in a weak light, as the image is faintly visible after exposure under the transparency.

The composition of the five developers just used is as follows—and it is only right to say here that I am indebted for the formulæ of these solutions to two photographic journals, *The British Journal of Photography* and *Anthony's Photographic Bulletin*:

## DEVELOPERS.

*No. 1—Red.*

Betanaphthol, . . . . .	30 grains.
Caustic soda, . . . . .	40 grains.
Water, . . . . .	10 ounces.

*No. 2—Yellow.*

Crystallized carbolic acid, . . . . .	50 grains.
Water, . . . . .	10 ounces.

*No. 3—Orange.*

Resorcin, . . . . .	30 grains.
Water, . . . . .	10 ounces.
Caustic soda, . . . . .	45 grains.

*No. 4—Purple.*

Naphthylamin, . . . . .	60 grains.
Water, . . . . .	10 ounces.
Oxalic acid, . . . . .	6 grains.

*No. 5—Black.*

Eikonogen, . . . . .	50 grains.
Water, . . . . .	10 ounces.

Transparencies on glass, and positives on celluloid and opal, can be produced as shown by the specimens I have here, and which can be inspected at the close of the meeting.

The composition of the coating is as follows:

Water (distilled) . . . . .	20 ounces.
Gelatine, . . . . .	1 ounce.
Primuline, . . . . .	80 grains.
Chrome alum, . . . . .	2 grains.



Put the gelatine to soak in six ounces of water, dissolve the primuline in twelve ounces of hot distilled water, and when dissolved add the gelatine and the chrome alum—dissolved in the remaining two ounces of water—filter through cotton, and flow while warm on the glass or porcelain. When dry immerse for two minutes in the sensitizing solution, wash for five minutes and allow to dry in the dark. The operation of printing and developing is the same as for prints, or cotton, etc.

PROCEEDINGS  
OF THE  
ELECTRICAL SECTION,  
OF THE  
FRANKLIN INSTITUTE.

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*[Proceedings of the stated meeting of the Electrical Section, held Tuesday,  
May 5, 1891.]*

HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, May 5, 1891.

Prof. EDWIN J. HOUSTON, President, in the chair.

Present, forty-eight members and visitors.

The minutes of the previous meeting were read and approved.

The treasurer presented bills for printing and clerical work, which were ordered paid when approved.

The committee on admissions reported four regular and ten associate members elected since last meeting.

The committee appointed to consider the subject of re-wiring the Institute building, presented a report suggesting certain changes in the present wiring, and stating the cost at which they could be made. The report was accepted and referred to the board of managers.

Three nominations to membership were referred to the committee on admissions.

Mr. David Brooks presented a communication on "The Value of Oil as an Insulator for High Voltage Currents," illustrated with some brilliant experiments. It was referred for publication.

Prof. Henry Crew gave a description of "Ewing's Theory of Induced Magnetism," illustrated with a model made after his suggestions, and with charts of typical magnetic phenomena that may be explained on this hypothesis. The communication was discussed by Prof. Houston and Mr. Pike, and referred for publication.

Mr. John Hoskin read a paper on "A Rough-and-Ready Dynamometer." Referred for publication.

There was considerable discussion on the contents of the Question-Box.

The meeting then adjourned.

L. F. RONDINELLA, *Secretary*.

## A ROUGH-AND-READY DYNAMOMETER FOR SMALL MOTORS.

BY JOHN HOSKIN.

[*Read at the meeting of the Electrical Section, held May 5, 1891.*]

I have been requested to bring to your notice this evening a friction-brake dynamometer that is by no means new, but is one that is deserving of being more widely known by electricians than appears to be the case.

Like the well-known Prony brake it acts as an absorption dynamometer; and without detracting from the acknowledged value of this instrument, especially for testing the value of large prime-movers, yet we need something more portable, more convenient to use, in the very numerous cases where it is desirable to test the efficiency of small motors.

We need a rough-and-ready instrument that is portable, inexpensive, readily used, and at the same time reliable.

This we find in a friction-brake dynamometer, one of the many modifications of those illustrated by Mr. William Worby Beaumont, in his paper on friction-brake dynamometers, read before the Institution of Civil Engineers in London, November 13, 1888, and published in 1889 in the *Proceedings* of the Institution.

Its construction requires only the use of a leather belt with a spring balance attached to one end, and a suitable weight at the other. The belt is to be thrown over the belt pulley of the motor, the spring balance is fastened to the floor base, or support of the motor to be tested, and the weighted end hangs pendant on the side of the pulley which, when in motion, will tend to lift the weight. When the motor is at rest, the strain of the weight should be read off on the spring balance. This reading we will call *W*. When the current is switched on and the motor runs at speed, the spring balance should again be read off, since the friction of the pulley on the belt will have a tendency to raise the weight; this reading we will call *W'*. The difference

between  $W$  and  $W'$  in pounds, multiplied by the circumference of the pulley in feet (including one-half the belt thickness on each side), and this by the number of pulley revolutions per minute will give the foot-pounds of mechanical energy, which can be compared with the electrical energy required to produce it, in the usual manner.

Thus, in a few minutes with the aid of a speed counter or tachometer, a voltmeter and an ammeter, the efficiency of a motor can be determined, and its ability to do a desired amount of work ascertained at once, instead of being left to guess-work, as would often be the case if more bulky or elaborate apparatus were needed.

It should not be imagined from these remarks that this form of dynamometer is applicable to small motors only; but it is evident that for testing larger machines where many horse-power have to be measured, the apparatus needs more elaboration, especially in the use of friction blocks under the belt or pulley strap, their lubrication, and the use of a dash-pot to steady the brake when the motive-power is irregular.

But I need not enlarge on this, except to say that in these particulars, whether using steel strap, or leather belt with friction blocks, or rope friction, the same care is needed as in the Prony brake to obtain correct readings.

Before concluding, I submit to your consideration the value of the Waldron rotary pump for use as a dynamometer, although I have never heard that it has been applied to that purpose. In examining one of these engines recently for other purposes it occurred to me that it would serve as an excellent dynamometer.

This pump is operated by rotary piston blades working in a chamber, without leakage, and with but small friction, the amount of which can readily be ascertained and calculated for use as a "constant." Its capacity per revolution, and the number of revolutions being known, as also the pressure against which it works, which can be regulated by a pressure gauge, the foot-pounds of work are at once arrived at.

This method of measurement will correctly register the

work done, although the speed may be irregular; and its results can be made more accurate than that of the friction dynamometer, because not subject to the irregularities arising from differences in lubricants, temperature, etc., which makes it necessary to use adjusting screws in most forms of friction-brake dynamometers.

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## THE VALUE OF OIL AS AN INSULATOR FOR HIGH VOLTAGE CURRENTS.

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BY DAVID BROOKS.

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[Read before the Electrical Section, May 5, 1891.]

Even as late as 1884, in the electrical works, oils were placed among conductors. For instance, James T. Sprague, in the second edition of his book, places them among conductors "in the order of their specific gravity." His first patent for the use of oil as an insulator was taken out in 1878, and now the matter is receiving great attention among the electricians of Europe. In the *London Electrical Review* there is an account of tests by Siemens and Halske, of Berlin, in which they state that an ordinary Siemens electric light cable was inserted, and broke down at a pressure of some 15,000 volts, but when oil was used, it perfectly withstood the pressure as high as 20,000 volts.

Oil has been used in Switzerland with complete success, not only for insulating wires, but also for transformers.

The writer has made the following experiment to show how much better oil is than the air for insulation. A large Holtz machine that sparked 7.5 inches through the air was unable to puncture the insulation of a cable of twisted wires separated one-fourth of an inch, when immersed in oil.

The ends of the twisted wires, were spliced to rubber-covered wires, and these were attached to the poles of the Holtz machine, after glass tubes had been drawn over them. The twisted wires were then placed in a jar contain-



ing oil with a layer of water floating on it, the oil being heavier than the water.

The spark broke through the tubes from wire to wire above the oil; the part of the jar containing water the tubes were broken opposite the splice, while the twisted wires in the oil stood the entire pressure of the machine, 7.5 inches spark. Such a spark would be equivalent to 175,000 volts, as defined by Warren De La Rue, between a point and a disk, but this spark of 7.5 inches was between two spheres which requires a much higher voltage.

When no glass tubes were used to protect the conductors, the voltage or tension was much reduced by silent or brush discharge. blue sparks passing through the rubber covering. When bare wires were used to connect the two poles of the Holtz machine, still less tension was produced on account of leakage through the air.

The experiment tends to show that the air is not a good insulator for very high voltages, and also to confirm the experiments made in Switzerland as to the value of oil for insulating transformers, and protecting them from destruction by lightning.

Experiments have also been made with heavily insulated electric light cables for conveying high voltages. None of them stood a tension above 12,000 volts, and most of them broke down by the use of less than 10,000 volts.

BOOK NOTICES.

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*A Treatise on Electro-Metallurgy*: Embracing the application of electrolysis to the plating, depositing, smelting and refining of various metals, and to the reproduction of printing surfaces and art-work, etc. By Walter G. McMillan, F.I.C., F.C.S., etc. With numerous illustrations. London: Charles Griffin & Co. Philadelphia: J. B. Lippincott Company. 1891. (8vo, pp. xv-387; price, \$3.50.)

The present volume forms a timely contribution to the literature—by no means voluminous—of electro-metallurgy. The author's aim has been judiciously to combine theory and practice, to enable the practical plater and refiner to acquire a knowledge of the fundamental principles of his art, by which to control and guide their practice, and to improve it along correct lines. To this end, the author's preliminary chapters, embracing an exposition of general principles, sources of current, and the general conditions to be observed in electro-deposition, are most excellent.

The chapters on the arrangement and disposition of the plant, preparation of work, as also those treating of the deposition of specific metals, give, without needless repetition, the most approved rules of practice and the results of the most recent work of specialists in this branch of applied electricity. The subject of industrial electro-metallurgical operations, which is at present attracting much attention, might have been treated at greater length, and would have added substantially to the value of the book, but that which is given is well-digested and conveys much information in limited space. On the whole, Mr. McMillan's book will be found to present the subjects with which it deals in very convenient form, and with commendable clearness and accuracy.

W. H. W.

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*A Move for Better Roads*. Essays on roadmaking and maintenance and road laws, for which prizes of honorable mention were awarded through the University of Pennsylvania, by a Committee of Citizens of Philadelphia, with a synopsis of other contributions and a review by the Secretary, Lewis M. Haupt, A.M., C.E., Professor of Civil Engineering, University of Pennsylvania, also an introduction by Wm. H. Rhawn, Chairman of the Committee. Philadelphia: University of Pennsylvania Press. 1891.

Our young country seems now to have reached that age (naturally delayed by the advent and development of railroads) when attention begins to be bestowed upon the condition of its highways. From all quarters come welcome evidences of a general awakening in this respect. The manufacturers of bicycles, so far from fearing to show their hand, come boldly forward as prominent champions of the movement. Difficult as we may find it to persuade ourselves that these gentlemen have lost sight of their personal interests in pushing this agitation, we may well afford to rejoice that the interests of a limited class are found to be here identical with those of the community at large.

In his introduction to this series of essays, Mr. Rhawn, the Chairman of the Citizens' Committee who awarded the prizes, refers to the encouragement extended to the committee by Dr. Wm. Pepper, the Provost, and Prof. Lewis M. Haupt, Professor of Civil Engineering of the University, and expresses the obligations of the committee to the authors of the several papers contributed.

The committee were moved to their action by the belief that the chief difficulty in the way of improvement is ignorance on the part of our people as to what a good road is, and it is with a view to doing something to dispel this ignorance that they solicited these papers and offered their liberal prizes, of \$400, \$200 and \$100, respectively, with honorable mention (at the discretion of the Board of Adjudicators) for other contributions.

The first prize was awarded to Henry Irwin, B.A., C.E., Canadian Pacific Railway, Montreal; the second to David H. Bergey, B.Sc., M.D., North Wales, Penna.; the third to James Bradford Olcott, South Manchester, Conn.; and honorable mention, without reference to order, to Edwin Satterthwait, Jenkintown, Pa.; Charles Punchard, 1223 Hollywood Avenue, Philadelphia; George B. Fleece C.E., Memphis, Tenn.; Frank Cawley, B.S., Swarthmore College, Penna.; and Francis Fuller McKenzie, C.E., 5774 Germantown Ave., Philadelphia.

The papers of these gentlemen, naturally covering much the same ground, are here presented, and followed by a Digest by Prof. Haupt of the contents of the remaining contributions, abstracts from miscellaneous communications received upon the general subject, notes by the Secretary, Prof. Haupt, and a copy of the Rules of the Road Improvement Association, of London.

In his notes, the Secretary endeavors to disabuse the agricultural mind of the impression "that the cost of transforming mud roads into macadam is so great as to be impracticable." Adding \$4,000,000 estimated as wasted annually "in Pennsylvania alone in consequence of her poor and often impassable roads" to an equal sum raised by road taxes and wasted in "useless attempts to build a permanent structure out of incoherent, perishable materials over improper locations," we have an annual loss, through bad roads, of \$8,000,000.

He points out that road improvement is obstructed not only by "ignorance as to the benefits to result from it," but also by "distrust as to the integrity of those who may be selected or appointed to conduct the work and handle the means;" and urges the employment in this work of "the civil engineers and surveyors resident in the great Commonwealth of Pennsylvania, whose reputation for skill and honesty is worth more to them than money, and who are deeply interested as citizens in promoting this long-needed reformation."

As to the raising of means for road improvement, the Secretary favors "State aid under proper restrictions," but "no public work of this character should be undertaken until a competent civil engineer is appointed to represent the interests of the State." The abolition of the system of "working out" road taxes is strongly advocated.

After brief remarks touching the dimensions and construction of roads, the Secretary concludes his notes by discussing "Soils and their adaptation to roads," in which he urges the importance of locating roads upon suitable ground, and gives data respecting the shrinkage, absorption and saturation of various soils, their behavior under changes of temperature and their rates of drying.

The Board of Adjudicators recommended to the committee, irrespective of the relative merits of the papers selected for publication :

(1) That in the improvement of roads, the macadam system, consisting of small angular fragments, in sizes not exceeding from two to two and one-half inches in their largest dimension, according to the quality, should be used, wherever a stone surface is both practical and justifiable.

(2) That the minimum width of the metalled surface for a single track should be a demiroad (eight and one-quarter feet), of such depth as the amount of traffic and character of the subsoil may require, to be determined by the engineer in charge.

(3) The bed to receive the stone must be so prepared that it cannot be saturated with water, and to accomplish this, great attention should be paid to the character and drainage of the subsoil.

(4) That there should be legislative enactment regulating the width of the tires of wheels, and that the minimum width of all cart, dray, wagon and other heavy-draught vehicles should be four inches, to be increased when the capacity of the vehicle exceeds half a net ton per wheel, at the rate of one inch for each 400 pounds in excess.

T.

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*A Text-book on Roofs and Bridges.* Part II, Graphic Statics. By Mansfield Merriman, Professor of Civil Engineering in Lehigh University, and Henry S. Jacoby, Instructor in Civil Engineering in Lehigh University, New York: John Wiley & Sons. 1890.

This volume represents the second of the four parts into which the course in roofs and bridges at Lehigh University is divided. Part I, treating of stresses in simple trusses, appeared in 1888, and it is much to be hoped that in due time the entire course will be completed by the publication of the two remaining portions, viz: Part III, the design of a bridge, with details and the preparation of working drawings; and Part IV, the discussion of cantilever, suspension and arched bridges.

The works forming this series make no pretensions to novelty, except in their arrangement and in the manner of presentation of the subject; but the authors, in their preface to Part II, invite attention to the abbreviated process employed in some of the diagrams for wind stresses, to the method of determining stresses due to initial tension, and to portions of the analysis of maximum moments and shears under locomotive wheels, as being in some respects novel as well as of practical value.

The present volume, the result of the combined labors of Professor Merriman and Instructor Jacoby, bears evidence of the same ability and conscientiousness which have marked the productions of the author first named. The

typography is excellent, and it seems unfortunate that so exemplary a work should be marred by faulty illustrations. Many of the cuts are rendered annoyingly indistinct by being unnecessarily crowded, and blurred by carelessness in their preparation or printing, or both.

In a text-book like this, intended to be studied as a whole from end to end, the absence of an index may perhaps be more readily condoned than in a book of reference, to be consulted occasionally for information on special subjects. T.

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## Franklin Institute.

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[*Proceedings of the stated meeting, held Wednesday, May 20, 1891.*]

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, May 20, 1891.

JOS. M. WILSON, President, in the chair.

Present, 220 members and twenty-six visitors.

Additions to membership since last report, eleven.

Mr. S. Lloyd Wiegand, from the Committee on Science and the Arts, presented an abstract of the investigation and conclusions of the committee in the case of the method of manufacturing truck-wheel centres invented by Mr. Samuel M. Vaucelain, and carried on extensively at the Baldwin Locomotive Works.

Prof. Coleman Sellers, by invitation, gave an excellent description, with illustrations, of the plans of the Cataract Construction Company, for the utilization, to the extent of 120,000 horse-power, of the water-power of Niagara Falls. The speaker's remarks embraced also some account of observations upon subjects of engineering interest, made by him during a recent visit to Europe as one of the Board of Consulting Engineers, commissioned by the Company to examine and report upon the most recent methods and examples of power transmission, and as representing the United States on the International Niagara Commission and kindred subjects. (Referred for publication.)

The thanks of the meeting were voted to the speaker, with the request that he continue his remarks at the next stated meeting.

In acknowledging the action of the meeting, Prof. Sellers stated that though he might be unable to do so at the June meeting, he would take pleasure in addressing the Institute at some later date.

Mr. W. N. Jennings exhibited a second instalment of lantern views illustrative of characteristic local subjects. These were well received.

The Secretary presented a brief report. Adjourned.

WM. H. WAHL, *Secretary.*



# PENNSYLVANIA STATE WEATHER SERVICE.

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## MONTHLY WEATHER REVIEW.

FOR JANUARY, 1891.

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*Prepared under the Direction of the Committee on Meteorology of the  
Franklin Institute.*

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, January 31, 1891.

### TEMPERATURE.

The mean temperature of 54 stations for January, 1891, was  $30^{\circ}6$ , which is over  $4^{\circ}0$  above the normal, and  $7^{\circ}1$  below the corresponding month of 1890.

The mean of the daily maxima and minima temperatures  $37^{\circ}7$  and  $23^{\circ}3$  give an average daily range of  $14^{\circ}4$ , and a monthly mean of  $30^{\circ}5$ .

Highest monthly mean,  $36^{\circ}4$  at Philadelphia.

Lowest monthly mean,  $23^{\circ}8$  at Eagles Mere.

Highest temperature recorded during the month,  $64^{\circ}0$  on the 29th at Uniontown.

Lowest temperature, minus  $4^{\circ}0$  on the 4th at Eagles Mere and Lewisburg. on the 8th at Columbus, and on the 9th at Wellsboro.

Greatest local monthly range,  $20^{\circ}2$  at Lancaster.

Least local monthly range,  $10^{\circ}1$  at Pittsburg and Grampian Hills.

Greatest daily range,  $43^{\circ}$  at Smethport on the 25th.

Least daily range,  $2^{\circ}$  at Grampian Hills, on the 11th.

From January 1, 1891, to January 31, 1891, the excess in temperature at Philadelphia was  $122^{\circ}$ , at Erie  $137^{\circ}$ , and at Pittsburgh  $164^{\circ}$ .

## BAROMETER.

The mean pressure for the month, 30·06, is about ·04 below the normal. At the U. S. Signal Service Stations, the highest observed was 30·67, at Pittsburgh on the 8th, and the lowest, 29·22, at Philadelphia on the 12th.

## PRECIPITATION.

The average precipitation, 3·64 inches for the month, is nearly normal.

The largest totals in inches (including melted snow) were Girardville, 8·74; Coopersburg, 7·29 and Pottstown, 7·00.

The least were Greenville, 1·22; Chambersburg, 2·27; Altoona, 2·35; and Erie, 2·43.

The snowfall average for the State was about 9·00 inches.

The largest totals in inches were Somerset, 40·5; Blue Knob, 33·5; Dyberry, 20·0; Eagles Mere, 19·0; and Charlesville, 15·0.

Only elevated districts report snow on the ground at the end of the month.

## WIND AND WEATHER.

The prevailing wind was from the Northwest. The wind and snow storm of the 25th caused great damage to telegraph and telephone wires.

Average number: Rainy days, 12; clear days, 6; fair days, 10; cloudy days, 15.

NOTE.—Any one having extra copies of the Monthly Weather Review for the months of May and August, 1889, will confer a favor by returning them to the Philadelphia office.

## MISCELLANEOUS PHENOMENA.

*Thunder-storms.*—Charlesville, 29th; Blue Knob, 29th; Quakertown, 11th; Johnstown, 29th; Emporium, 29th; Mauch Chunk, 11th; State College, 29th; Coatesville, 11th; Phoenixville, 11th; Rimersburg, 29th; Grampian Hills, 29th; Lock Haven, 29th; Meadville, 29th; Carlisle, 29th; Swarthmore, 11th; Uniontown, 29th; McConnellsburg, 29th; Indiana, 28th; Lancaster, 11th; Coopersburg, 11th; Nisbet, 29th; Smethport, 30th; Somerset, 19th; Wellsboro, 29th; Columbus, 29th; Dyberry, 12th; Philadelphia, 11th; York, 12th; Hollidaysburg, 29th.

*Hail.*—Phoenixville, 17th, 31st; Meadville, 29th; Smethport, 30th; Somerset, 19th; Wellsboro, 1st, 2d; Dyberry, 17th; Philadelphia, 31st; York, 31st.

*Snow.*—Charlesville, 3d, 5th, 10th, 12th, 17th, 18th, 22d, 24th, 25th, 31st; Blue Knob, 2d, 3d, 4th, 5th, 6th, 7th, 9th, 19th, 12th, 13th, 14th, 17th, 18th, 22d, 23d, 24th, 25th, 26th; Wysox, 11th, 17th, 24th; Le Roy, 3d, 12th, 17th, 18th; Quakertown, 1st, 4th, 7th, 17th, 25th; Johnstown, 2d, 3d, 4th, 5th, 6th, 12th, 17th, 18th, 22d, 23d, 24th, 25th; Emporium, 12th, 13th, 14th, 22d, 23d, 26th, 27th; Mauch Chunk, 17th; State College, 4th, 6th, 12th, 13th, 17th, 18th, 24th, 25th; West Chester, 17th, 24th, 25th; Coatesville, 17th, 24th, 25th.

Phoenixville, 1st, 5th, 7th, 17th, 25th; Westtown, 25th; Rimersburg, 2d, 3d, 4th, 5th, 6th, 12th, 13th, 14th, 17th, 22d, 26th; Grampian Hills, 4th, 5th, 12th, 13th, 17th, 22d; Lock Haven, 17th, 18th, 24th, 25th; Catawissa, 17th, 18th, 25th; Meadville, 3d, 5th, 6th, 12th, 14th, 22d, 23d, 26th; Carlisle, 17th, 18th, 25th; Swarthmore, 25th; Uniontown, 2d, 4th, 5th, 6th, 7th, 12th, 13th, 14th, 23d; Huntingdon, 17th, 18th, 24th; Petersburg, 17th, 18th, 25th; Indiana, 2d, 4th, 5th, 6th, 7th, 10th, 12th, 24th, 25th, 30th; Lancaster, 6th, 17th, 18th; New Castle, 3d, 6th, 12th, 13th, 22d; Coopersburg, 1st, 17th, 18th, 25th; Wilkes-Barre, 1st, 18th, 25th; Nisbet, 17th, 18th, 25th, 27th; Smethport, 2d, 3d, 4th, 5th, 6th, 10th, 12th, 13th, 14th, 22d, 26th, 30th; Greenville, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 11th, 12th, 13th, 14th, 17th, 18th, 22d, 23d, 26th, 30th; Pottstown, 17th, 25th; Philadelphia, 5th, 18th, 25th; Girardville, 5th, 12th, 18th, 24th, 25th; Somerset, 1st, 3d, 5th, 6th, 7th, 12th, 13th, 17th, 18th, 25th; Eagles Mere, 3d, 5th, 7th, 12th, 14th, 17th, 18th, 23d, 25th; Wellsboro, 13th, 18th; Lewisburg, 14th, 17th, 18th, 25th; Dyberry, 1st, 2d, 3d, 4th, 5th, 6th, 12th, 13th, 14th, 17th, 18th, 23d, 25th, 26th, 28th, 29th, 30th, 31st; South Eaton, 14th, 17th, 18th, 25th; York, 17th, 25th; Hollidaysburg, 4th, 12th, 17th, 18th, 24th; Altoona, 17th, 18th, 22d, 25th; Kennett Square, 25th.

*Sleet*.—Charlesville, 31st; West Chester, 17th; Coatesville, 17th; Rimersburg, 19th; Lock Haven, 22d; Lancaster, 17th; Wellsboro, 1st, 2d; Dyberry, 1st, 11th, 17th, 31st; South Eaton, 17th; Philadelphia, 17th; York, 17th.

*Coronæ*.—Charlesville, 15th, 20th, 22d; Le Roy, 20th; Rimersburg, 15th, 16th, 22d, 24th, 28th; Greenville, 24th; Dyberry, 15th, 20th, 21st, 31st.

*Solar Halos*.—Le Roy, 4th, 13th; Eagles Mere, 9th, 13th; Dyberry, 28th; Philadelphia, 9th, 15th.

*Lunar Halos*.—Le Roy, 15th, 20th; State College, 15th, 18th; West Chester, 20th; Phoenixville, 15th; Meadville, 16th, 25th; Carlisle, 15th, 21st, 25th, 28th, 29th; Lancaster, 25th, 27th; Nisbet, 20th; Greenville, 15th; Eagles Mere, 15th, 20th; Wellsboro, 15th, 21st, 24th; Dyberry, 15th, 20th, 21st; South Eaton, 20th; Philadelphia, 15th, 20th.

*Meteors*.—State College, 3d; Coopersburg, 7th.

*Parheliæ*.—Wellsboro, 15th; Philadelphia, 23d.

*Zodiacal Lights*.—Charlesville, 29th; Dyberry, 9th.

#### WEATHER FORECASTS.

Percentage of local verifications of weather and temperature signals as reported by displaymen for January, 1891:

Weather, 80 per cent.

Temperature, 86 per cent.

## TEMPERATURE AND WEATHER SIGNAL DISPLAY STATIONS,

<i>Displayman.</i>	<i>Station.</i>
U. S. Signal Office, . . . . .	Philadelphia.
Wanamaker & Brown, . . . . .	"
Pennsylvania Railroad Company, . . . . .	"
Continental Brewing Company, . . . . .	"
Samuel Simpson, . . . . .	"
B. T. Babbitt, . . . . .	"
Western Meat Company, . . . . .	"
Neptune Laundry, . . . . .	"
C. W. Burkhardt, . . . . .	Shoemakersville.
A. N. Lindenmuth, . . . . .	Allentown.
C. B. Whitehead, . . . . .	Bradford.
Thomas F. Sloan, . . . . .	McConnellsburg.
J. H. Fulmer, . . . . .	Muncy.
W. T. Butz, . . . . .	New Castle.
Capt. A. Goldsmith, . . . . .	Quakertown.
Frank Ross, . . . . .	Oil City.
Lerch & Rice, . . . . .	Bethlehem.
Signal Office, . . . . .	Erie.
J. R. Raynsford, . . . . .	Montrose.
E. P. Wilbur & Co., . . . . .	South Bethlehem.
Agricultural Experiment Station, . . . . .	State College.
Signal Office, . . . . .	Pittsburgh.
<i>New Era</i> , . . . . .	Lancaster.
D. G. Hurley, . . . . .	Altoona.
J. E. Forsythe, . . . . .	Butler.
Steward M. Dreher, . . . . .	Stroudsburg.
State Normal School, . . . . .	Millersville.
E. C. Wagner, . . . . .	Girardville.
L. H. Grenewald, . . . . .	York.
J. E. Pague, . . . . .	Carlisle.
C. L. Peck, . . . . .	Coudersport.
H. D. Miller, . . . . .	Drifton.
M. Tannehill, . . . . .	Confluence.
S. C. Burkholder, . . . . .	Pollock.
Robt. M. Graham, . . . . .	Catawissa.
Henry F. Bitner, . . . . .	Millersville.
A. M. Wildman, . . . . .	Langhorne.
G. W. Klee, . . . . .	Chambersburg.
A. Simon's Sons, . . . . .	Lock Haven.
<i>Raftsmen's Journal</i> , . . . . .	Clearfield.
R. C. Schmidt & Co., . . . . .	Belle Vernon
Chas. B. Lutz, . . . . .	Bloomsburg.
E. C. Lorentz, . . . . .	Johnstown.
W. M. James, . . . . .	Ashland.
Miller & Allison, . . . . .	Punxsutawney.

<i>Displayman.</i>	<i>Station.</i>
E. J. Sellers, . . . . .	Kutztown.
H. M. Kaisinger, . . . . .	Hartsville.
Foulk & Co., . . . . .	Milford.
William Lawton, . . . . .	Wilmington, Del.
Wister Heberton & Co., . . . . .	Germantown.
Charles M. Mullen, . . . . .	Bedford.
E. W. Merrill, . . . . .	North East.
A. Simon's Sons, . . . . .	Lock Haven.
Frank Ridgway, . . . . .	Harrisburg.
G. W. Yost, . . . . .	Collegeville.
A. H. Gerbench, . . . . .	Annaville.
Knowles Croskey, . . . . .	Phoenixville.
Powell Bros., . . . . .	Shadeland.
Lincoln E. Rowley, . . . . .	Athens.
P. S. Weber, . . . . .	Du Bois.
E. Jennef, . . . . .	Franklin.
Milton C. Cooper, . . . . .	Ashbourne.
J. L. Long, . . . . .	Freeport.
J. H. Apple, . . . . .	Clarion.
George Wheeler, . . . . .	Doylestown.
J. C. McCloskey, . . . . .	Lock Haven.
Charles Petersen, . . . . .	Honesdale.
Armstrong & Brownell, . . . . .	Smethport.





ATE WEATHER SERVICE

STATION NAME	ELEVATION Feet	Dew Point	PRECIPITATION	
			Total Inches	Total Snowfall During Month
Allegheny	2	30.0	2.43	3.20
Bedford	10	23.7	2.63	15.00
Blair	4	28.5	2.35	
Blair	6		3.65	33.50
Blair	6	24.7	3.74	9.00
Bradford	5	23.6	3.72	12.55
Bradford			4.63	13.05
Bucyrus			6.11	
Bucyrus	5	25.9	6.26	10.00
Canton	4	27.0	3.47	4.50
Canton			3.47	1.00
Canton			6.90	5.50
Cen.	5	25.6	4.11	13.80
Chester	0	26.0	6.56	5.50
Chester			6.59	5.00
Chester			6.08	3.00
Chester	6	26.7	5.19	5.51
Chester			5.74	4.00
Clarksburg				4.50
Clarksburg				
Cle.			3.62	6.00
Clinch			4.21	12.00
Clinch			5.26	13.00
Col.	0	24.7	4.12	8.75
Crawford	0	26.0	4.07	9.75
Crawford	7	26.2	4.73	6.00
Del.	0	28.0	4.84	5.00
Del.	0	24.0	2.32	
Erie			3.41	2.00
Fairfax	5	27.1	2.27	5.00
Fairfax	5	26.0	4.53	15.00
Hunt			3.58	15.25
Hunt			4.29	9.00
Ind.	9	27.8	4.40	5.50
Lar.	9	26.5	3.09	3.50
Lav.	1	30.9	2.88	2.35
Lef.				
Lef.			7.29	7.90
Luz.				
Luz.			4.59	15.00
Luz.			4.00	9.60
Mc.				
Me.	8	27.0	1.22	8.00
Mil.	0	27.0	7.00	4.00
No.				
Phi.	0	27.0	3.65	2.50
Phi.	0	31.6	5.18	3.00
Sch.			8.74	22.00
Sny.			8.59	13.00
Sof.	8	27.1	5.51	40.50
Sul.	8	17.8	5.50	19.00
Tid.	3	23.6	6.53	5.00
Un.			3.33	12.00
W.	0	18.0	2.57	
W.				
W.			5.30	17.00
W.			4.21	20.00
W.			3.39	3.66
W.			5.47	11.00
Vo.	5	27.1	3.05	7.75

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	Lock Haven.	Mauch Chunk.	Meadville.	McConnellsburg.	New Castle.	Nisbet.
1	'04	'31	'85	..	'83	'1
1	'20	'74	'08	..	'05	'2
1	..	..	'20	..	..	..
1	..	..	'10	..	..	..
1	..	..	'05	..	'05	..
1	'01	*.	..	..	..	..
1	'82	'70	'68	'90	..	'8
1	'47	'33	'12	..	'49	'2
1	..	..	..	..	'02	..
1	..	..	'20	..	..	..
1	..	..	..	..	..	..
1	'20	..	..	'36	..	..
1	'80	'94	..	'15	..	'1
1	..	..	..	..	..	..
2	..	..	..	..	..	..
2	'93	'85	'58	'11	'40	..
2	..	..	'10	..	..	..
2	'10	..	..	..	..	..
2	'10	'03	..	'48	..	..
2	'05	..	'05	..	..	..
2	..	..	..	..	..	..
2	'05	..	..	'07	'09	..
2	'35	..	'37	'23	'24	..
3	'04	*.	'14	..	..	..
3	'05	*.	'60	'23	'71	..
4	'21	'90	'12	'53	'28	'4

## PRECIPITATION FOR JANUARY, 1891.

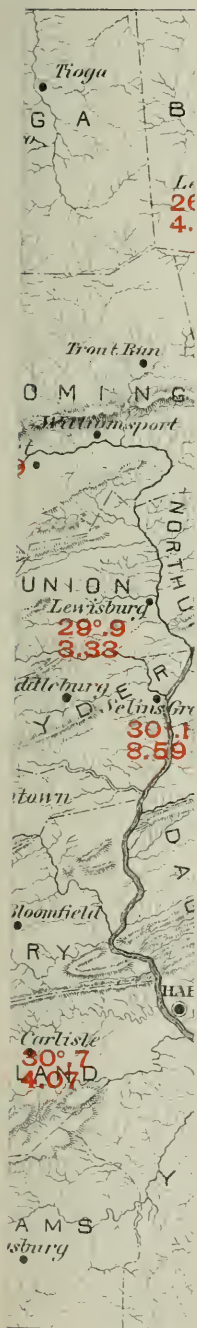
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\* No record.

† U. S. Signal Office.

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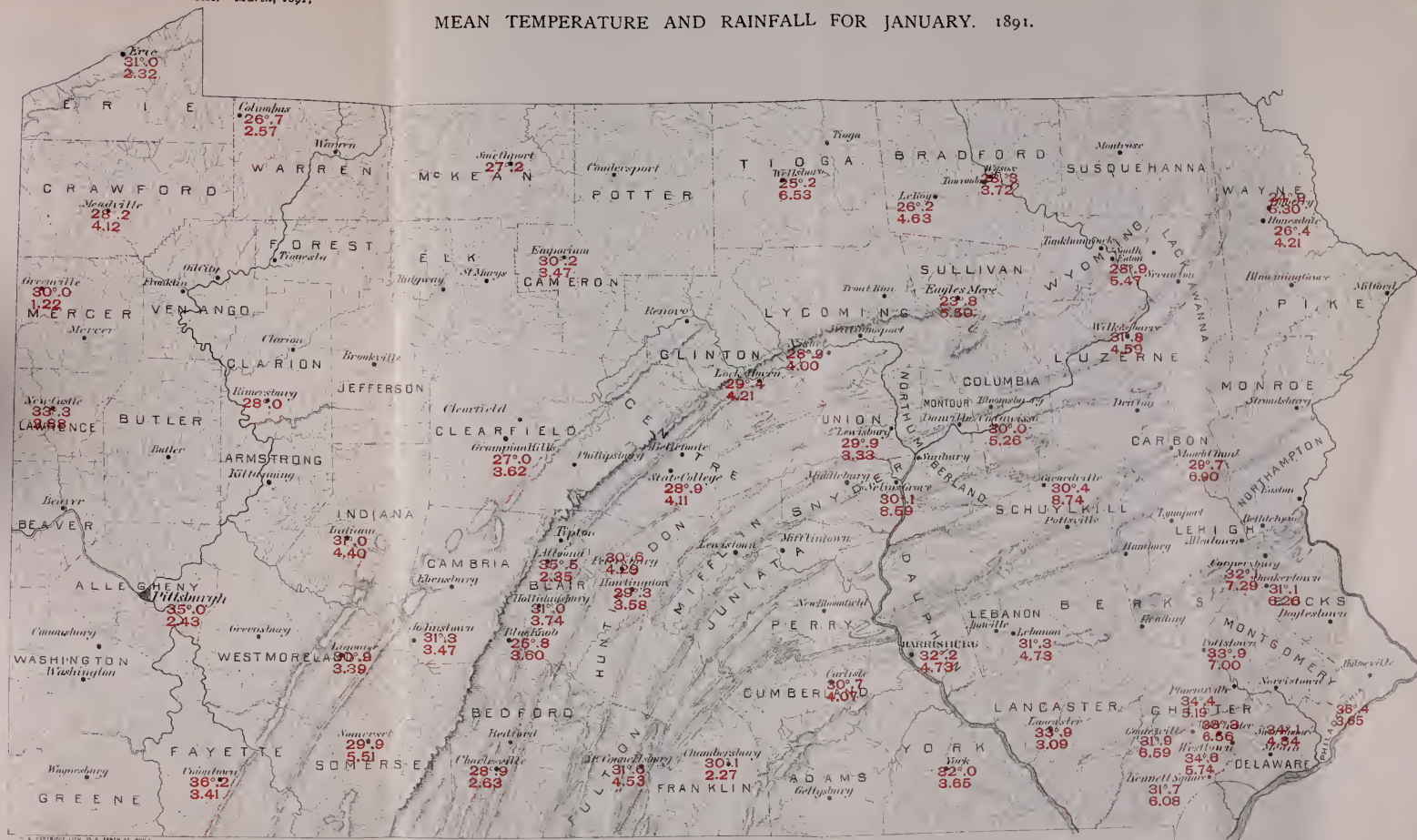
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MEAN TEMPERATURE AND RAINFALL FOR JANUARY. 1891.



# PENNSYLVANIA STATE WEATHER SERVICE.

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## MONTHLY WEATHER REVIEW.

FOR FEBRUARY, 1891.

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*Prepared under the Direction of the Committee on Meteorology of the  
Franklin Institute.*

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, February 28, 1891.

### TEMPERATURE.

The mean temperature of 55 stations for February, 1891, was  $34^{\circ}9$ , which is nearly  $6^{\circ}0$  above the normal, and  $2^{\circ}2$  below the corresponding month of 1890.

The mean of the daily maxima and minima temperatures  $42^{\circ}8$  and  $26^{\circ}7$  give an average daily range of  $16^{\circ}1$ , and a monthly mean of  $34^{\circ}7$ .

Highest monthly mean,  $40^{\circ}5$  at Philadelphia.

Lowest monthly mean,  $26^{\circ}8$  at Eagles Mere.

Highest temperature recorded during the month,  $70^{\circ}$  on the 18th at Philadelphia.

Lowest temperature, minus  $7^{\circ}$  on the 5th at Dyberry.

Greatest local monthly range,  $63^{\circ}$  at Somerset.

Least local monthly range,  $46^{\circ}$  at Kennett Square, Nisbet and Greenville.

Greatest daily range,  $43^{\circ}$  at Erie on the 5th.

Least daily range,  $2^{\circ}$  at State College and Grampian Hills on the 8th, Pottstown on the 7th, and Lock Haven on the 4th.

From January 1, 1891, to February 28, 1891, the excess in temperature at Philadelphia was  $244^{\circ}$ , at Erie  $274^{\circ}$ , and at Pittsburgh  $313^{\circ}$ .

The warmest days of the month were the 17th, 18th and 25th. The coldest, the 4th and 5th.

### BAROMETER.

The mean pressure for the month,  $30^{\circ}07$ , is about  $\cdot 03$  below the normal. At the U. S. Signal Service Stations, the highest observed was  $30^{\circ}71$ , at Philadelphia, on the 15th, and the lowest,  $29^{\circ}25$ , at Erie, on the 25th.

### PRECIPITATION.

The average precipitation,  $4^{\circ}61$  inches for the month, is slightly above the normal. The largest totals in inches (including melted snow) were Ligonier,  $8^{\circ}29$ ; Johnstown,  $7^{\circ}99$ ; Grampian Hills,  $7^{\circ}01$ ; Columbus,  $6^{\circ}67$ ; Meadville,  $6^{\circ}65$ ; Uniontown,  $6^{\circ}64$ , and Pittsburgh,  $6^{\circ}09$ .

The least were Blue Knob,  $2^{\circ}13$ ; Greenville,  $2^{\circ}50$ ; Chambersburg,  $2^{\circ}54$ ; Wysox,  $2^{\circ}71$ , and Drifton,  $2^{\circ}90$ .

The snowfall was light outside of the more elevated sections. Dyberry reports 17·5 inches; Blue Knob, 16·0; Nisbet, 15·0; Grampian Hills, 13·5; Eagles Mere, 12·5; and Le Roy, 12·3. Several stations report snow on the ground at the end of the month. Heavy rains occurred on the 16th and 17th, which caused heavy freshets in many parts of the state.

#### WIND AND WEATHER.

The prevailing wind was from the Northwest.

Average number: Rainy days, 12; clear days, 6; fair days, 8; cloudy days, 14.

CORRECTION.—For January. Greatest monthly range, 55·0 at Somerset. Least local monthly range 34·0 at Chambersburg.

#### MISCELLANEOUS PHENOMENA.

*Thunder-storms.*—Charlesville, 25th; Blue Knob, 25th; Johnstown, 25th; Westtown, 28th; Meadville, 25th; Uniontown, 17th; Greenville, 9th; Columbus, 25th; York, 1st.

*Hail.*—Quakertown, 26th; Phoenixville, 6th, 12th, 20th; Indiana, 1st; Lancaster, 12th, 19th, 20th; Wilkes-Barre, 9th; Nisbet, 9th, 18th, 20th; Columbus, 25th; Dyberry, 9th, 20th; York, 13th.

*Snow.*—Charlesville, 10th, 19th, 20th, 26th, 27th, 28th; Blue Knob, 3d, 7th, 10th, 11th, 20th, 21st, 22d, 26th, 27th, 28th; Hollidaysburg, 7th, 10th, 26th, 27th, 28th; Wysox, 7th, 19th, 20th, 28th; Le Roy, 4th, 7th, 9th, 20th, 27th, 28th; Quakertown, 6th, 7th, 26th, 27th; Johnstown, 10th, 22d, 27th, 28th; Emporium, 7th, 20th, 28th; State College, 4th, 7th, 9th, 10th, 20th, 28th; Coatesville, 26th; Kennett Square, 26th; Phoenixville, 26th, 27th; Rimersburg, 4th, 10th, 22d, 26th, 28th; Grampian Hills, 7th, 28th; Lock Haven, 9th, 20th, 28th; Meadville, 3d, 4th, 20th, 22d, 26th, 27th, 28th; Carlisle, 26th, 28th; Uniontown, 10th, 19th, 26th, 28th; Chambersburg, 26th; Huntingdon, 20th, 28th; Indiana, 10th, 26th, 27th, 28th; Lancaster, 26th, 28th; New Castle, 26th, 27th, 28th; Lebanon, 20th, 26th, 28th; Coopersburg, 20th, 26th; Wilkes-Barre, 9th, 20th, 26th, 28th; Nisbet, 1st, 7th, 9th, 20th, 28th; Smethport, 7th, 10th, 20th, 28th; Greenville, 3d, 4th, 7th, 8th, 10th, 18th, 20th, 21st, 22d, 26th, 27th, 28th; Pottstown, 26th; Philadelphia, 20th, 26th, 27th; Girardville, 7th, 20th, 26th; Somerset, 10th, 19th, 20th, 27th, 28th; Eagles Mere, 4th, 7th, 10th, 20th, 28th; Wellsboro, 7th, 9th, 20th, 28th; Lewisburg, 7th, 20th, 27th, 28th; Columbus, 1st, 2d, 3d, 4th, 7th, 10th, 14th, 20th, 22d, 26th, 27th, 28th; Dyberry, 7th, 8th, 9th, 20th, 26th, 27th, 28th; Ligonier, 27th, 28th; South Eaton, 8th, 20th, 28th; York, 26th, 28th.

*Sleet.*—Charlesville, 19th, 20th; Blue Knob, 3d; Hollidaysburg, 20th; Le Roy, 9th, 20th; Quakertown, 6th; Johnstown, 19th; Coatesville, 6th, 20th, 21st; Rimersburg, 19th, 20th; Grampian Hills, 20th; Lock Haven, 7th; Carlisle, 9th, 12th, 20th; Lebanon, 20th; Wilkes-Barre, 9th; Nisbet, 7th, 9th, 20th; Smethport, 1st; Philadelphia, 20th, 26th; Somerset, 20th; Dyberry, 9th, 20th, 21st; South Eaton, 9th, 20th; York, 20th.

*Aurora.*—Eagles Mere, 11th.

*Coronæ*.—Charlesville, 16th, 17th, 18th; Blue Knob, 11th; Rimersburg, 27th; Lebanon, 11th, 14th, 15th, 16th, 18th, 19th, 22d; Dyberry, 15th.

*Solar Halos*.—Le Roy, 5th, 19th, 23d, 24th; Lebanon, 24th; Philadelphia, 19th, 23d, 24th, 28th; Eagles Mere, 5th, 6th, 23d; Dyberry, 19th, 23d.

*Lunar Halos*.—Charlesville, 11th, 14th; Phoenixville, 15th, 16th, 22d, 23d; Meadville, 13th, 15th, 19th, 21st; Carlisle, 15th, 18th; Lancaster, 22d; Lebanon, 15th; Nisbet, 18th; Greenville, 22d; Philadelphia, 15th, 16th, 22d; Dyberry, 15th.

*Meteors*.—Charlesville, 27th; Philadelphia, 1st.

*Zodiacal Lights*.—Charlesville, 5th; Coatesville, 4th, 8th.

#### WEATHER FORECASTS.

Percentage of local verifications of weather and temperature signals as reported by displaymen for January, 1891:

Weather, 88 per cent.

Temperature, 88 per cent.

#### TEMPERATURE AND WEATHER SIGNAL DISPLAY STATIONS.

<i>Displayman.</i>	<i>Station.</i>
U. S. Signal Office, . . . . .	Philadelphia.
Wanamaker & Brown, . . . . .	"
Pennsylvania Railroad Company, . . . . .	"
Continental Brewing Company, . . . . .	"
Samuel Simpson, . . . . .	"
B. T. Babbitt, . . . . .	"
Western Meat Company, . . . . .	"
Neptune Laundry, . . . . .	"
C. W. Burkhardt, . . . . .	Shoemakersville.
A. N. Lindenmuth, . . . . .	Allentown.
C. B. Whitehead, . . . . .	Bradford.
Thomas F. Sloan, . . . . .	McConnellsburg.
J. H. Fulmer, . . . . .	Muncy.
W. T. Butz, . . . . .	New Castle.
Capt. A. Goldsmith, . . . . .	Quakertown.
Frank Ross, . . . . .	Oil City.
Lerch & Rice, . . . . .	Bethlehem.
Signal Office, . . . . .	Erie.
J. R. Raynsford, . . . . .	Montrose.
E. P. Wilbur & Co., . . . . .	South Bethlehem.
Agricultural Experiment Station, . . . . .	State College.
Signal Office, . . . . .	Pittsburgh.
<i>New Era</i> , . . . . .	Lancaster.
D. G. Hurley, . . . . .	Altoona.
J. E. Forsythe, . . . . .	Butler.



<i>Displayman.</i>	<i>Station.</i>
Steward M. Dreher, . . . . .	Stroudsburg.
State Normal School, . . . . .	Millersville.
E. C. Wagner, . . . . .	Girardville.
L. H. Grenewald, . . . . .	York.
J. E. Pague, . . . . .	Carlisle.
C. L. Peck, . . . . .	Coudersport.
H. D. Miller, . . . . .	Drifton.
M. Tannehill, . . . . .	Confluence.
S. C. Burkholder, . . . . .	Pollock.
Robt. M. Graham, . . . . .	Catawissa.
Henry F. Bitner, . . . . .	Millersville.
A. M. Wildman, . . . . .	Langhorne.
G. W. Klee, . . . . .	Chambersburg.
A. Simon's Sons, . . . . .	Lock Haven.
<i>Raftsmen's Journal</i> , . . . . .	Clearfield.
R. C. Schmidt & Co., . . . . .	Belle Vernon.
Chas. B. Lutz, . . . . .	Bloomsburg.
E. C. Lorentz, . . . . .	Johnstown.
W. M. James, . . . . .	Ashland.
Miller & Allison, . . . . .	Punxsutawney.
E. J. Sellers, . . . . .	Kutztown.
H. M. Kaisinger, . . . . .	Hartsville.
Foulk & Co., . . . . .	Milford.
William Lawton, . . . . .	Wilmington, Del.
Wister Heberton & Co., . . . . .	Germantown.
Charles M. Mullen, . . . . .	Bedford.
E. W. Merrill, . . . . .	North East.
A. Simon's Sons, . . . . .	Lock Haven.
Frank Ridgway, . . . . .	Harrisburg.
G. W. Yost, . . . . .	Collegeville.
A. H. Gerbench, . . . . .	Annaville.
Knowles Croskey, . . . . .	Phoenixville.
Powell Bros., . . . . .	Shadeland.
Lincoln E. Rowley, . . . . .	Athens.
P. S. Weber, . . . . .	Du Bois.
E. Jennet, . . . . .	Franklin.
Milton C. Cooper, . . . . .	Ashbourne.
J. L. Long, . . . . .	Freeport.
J. H. Apple, . . . . .	Clarion.
George Wheeler, . . . . .	Doylestown.
J. C. McCloskey, . . . . .	Lock Haven.
Charles Petersen, . . . . .	Honesdale.
Armstrong & Brownell, . . . . .	Smethport.

COUN

Date.

Allegheny	10	7
Bedford,	10	7
Blair, <sup>2</sup>	20	7
Blair,	.	.
Blair,	7	7
Bradford,	4	7
Bradford,	2	.
Bucks,	.	.
Bucks,	7	8
Cambria, <sup>1</sup>	7	8
Cameron,	10	.
Carbon, <sup>1</sup>	8	.
Centre,	.	.
	8	8
Chester,	7	.
Chester,	7	.
Chester, <sup>1</sup>	.	.
Chester,	7	.
Chester,	7	.
Clarion,	7	.
Clarion,	.	.
	.	7
Clearfield,	8	.
Clinton,	4	.
Columbia,	.	.
Crawford,	1	.
Cumberland,	7	.
Danphin, <sup>1</sup>	9	.
Delaware,	.	.
	7	.
Erie, <sup>1</sup>	26	.
Fayette,	10	.
Franklin, <sup>1</sup>	.	.
	20	.
Fulton,	7	.
Huntingdo	.	.
	27	.
Huntingdo	.	.
Indiana,	.	.
	1	.
Lancaster	.	.
	10	.
Lawrence,	21	.
Lebanon,	9	.
Lehigh,	7	.
Luzerne,	.	.
	25	.
Luzerne, <sup>1</sup>	25	.
Lycoming,	.	.
McKean,	26	.
Mercer, <sup>1</sup>	.	.
	8	.
Mifflin,	7	.
Northamp.	.	.
Philadelphi	.	.
	27	.
Philadelphi	12	.
Schuylkill	25	.
Snyder,	.	.
Somerset,	3	.
Sullivan,	26	.
Tioga,	9	.
Union, <sup>1</sup>	10	.
Warren,	22	.
Washingt.	.	.
Wayne,	20	.
Wayne,	16	.
Westmore	26	.
Wyoming	8	.
York, <sup>1</sup>	.	.

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MONTHLY SUMMARY OF REPORTS BY VOLUNTARY OBSERVERS OF THE PENNSYLVANIA STATE WEATHER SERVICE FOR FEBRUARY, 1891.

COUNTY.	STATION.	Elevation above Sea Level (feet).	BAROMETER REDUCED TO SEA LEVEL.			TEMPERATURE.										PRECIPITATION.				NUMBER OF DAYS.			WIND.			OBSERVERS.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																			
			Mean.	Highest.	Lowest.	MAXIMUM.		MINIMUM.		DAILY RANGE.										Clear.	Fair.	Cloudy.	PREVAILING DIRECTION.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																						
						Mean.	Highest.	Date.	Lowest.	Date.	Mean of Maximum.	Mean of Minimum.	Mean.	Greatest.	Date.	Least.	Date.	Relative Humidity.	Dew Point.				Total inches.	Total Snowfall during Month.	Depth of Snow on Ground at End of Month.		Number of Days Rainfall.		7 A. M.	8 P. M.	9 P. M.																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																																														

	Altoona.	Bethlehem.	McConnellsburg.	New Castle.	Nisbet.	Ottsville.	Distances.
1	'18	..	'30	'08	..	'40	.
2	'04	..	..	..	..	..	.
3	'15	..	'20	'02	'20	'25	.
4	..	..	..	..	..	..	.
5	..	..	..	..	..	..	.
6	..	..	..	..	..	..	.
7	'10	..	1'09	'41	1'00	'23	.
8	'87	..	..	'20	..	'23	.
9	'39	..	'72	'13	'70	'44	.
0	'37	..	..	..	..	'11	.
1	..	..	..	..	..	..	.
2	..	..	..	..	..	..	.
3	..	..	..	..	..	..	.
4	..	..	..	..	..	..	.
5	'73	..	..	..	..	..	.
6	'95	..	'93	..	..	'61	.
7	'17	..	'30	1'77	1'70	'30	.
8	..	..	..	..	'20	'02	.
9	'40	..	..	..	..	..	.
0	'10	..	..	'56	'70	'28	.
1	..	..	1'05	'35	'30	'20	.
2	*	..	..	..	..	..	.
3	..	..	..	..	..	..	.
4	..	..	..	'03	..	..	.
5	..	..	..	'21	'20	'21	.
6	'05	..	..	'04	..	'61	.
7	..	..	..	..	..	'13	.
8	*	..	'05	'07	'20	..	.
9	..	..	..	..	..	..	.
0	..	..	..	..	..	..	.
1	..	..	..	..	..	..	.
<hr/>							
	4'59	..	5 4'64	3'87	5'20	4'02	.

\* No recd

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## PRECIPITATION FOR FEBRUARY, 1891.

Albion,	18	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
Belknap,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Blue Knob,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Cañonburg,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Calais,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Cawassa,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Chambersburg,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Charleville,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Chilton,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Columbia,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Conestoga,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Copertburg,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Doylestown,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Dryden,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Dyersburg,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Eagles Mere,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Emporium,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Forbes of Neshaminy,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Frederick,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Germanown,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Granville,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100	
Granran Hills,	19	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	5																																													

\* No record.

† U. S. Signal Office.

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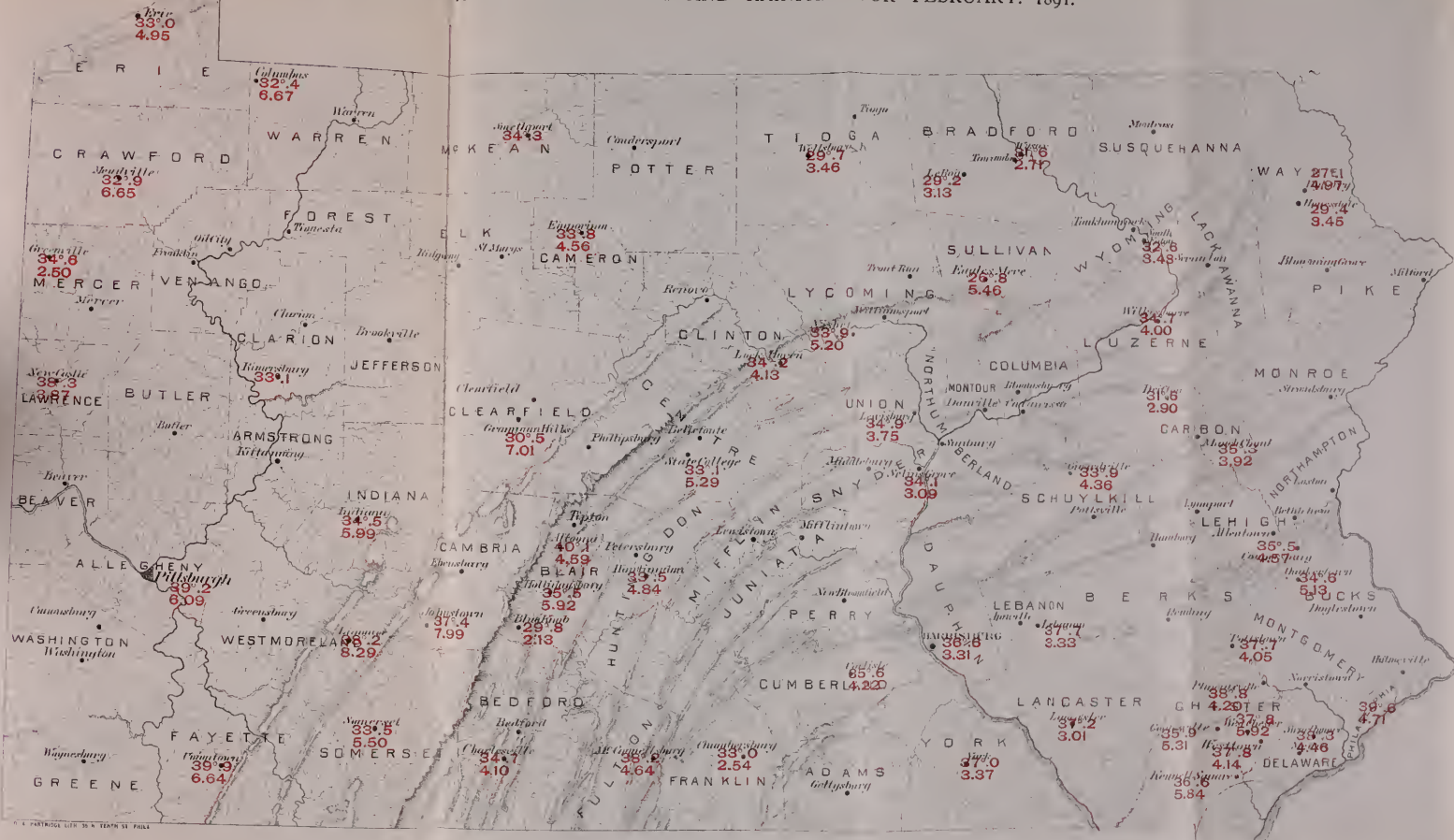
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# MEAN TEMPERATURE AND RAINFALL FOR FEBRUARY. 1891.



# PENNSYLVANIA STATE WEATHER SERVICE.

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## MONTHLY WEATHER REVIEW.

FOR MARCH, 1891.

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*Prepared under the Direction of the Committee on Meteorology of the  
Franklin Institute.*

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, March 31, 1891.

### TEMPERATURE.

The mean temperature of 53 stations for March, 1891, was  $34^{\circ}\cdot 1$ , which is nearly  $2^{\circ}\cdot 0$  below the normal, and  $0^{\circ}\cdot 7$  above the corresponding month of 1890.

The mean of the daily maxima and minima temperatures  $42^{\circ}\cdot 0$  and  $25^{\circ}\cdot 7$  give an average daily range of  $16^{\circ}\cdot 3$ , and a monthly mean of  $33^{\circ}\cdot 8$ .

Highest monthly mean,  $38^{\circ}\cdot 4$  at Philadelphia.

Lowest monthly mean,  $29^{\circ}\cdot 1$  at Eagles Mere.

Highest temperature recorded during the month,  $60^{\circ}\cdot 0$  on the 18th at Pittsburgh and Uniontown.

Lowest temperature, minus  $14^{\circ}$  on the 2d at Dyberry and Columbus.

Greatest local monthly range,  $70^{\circ}$  at Smethport and Dyberry.

Least local monthly range,  $43^{\circ}$  at Harrisburg.

Greatest daily range,  $42^{\circ}$  at Emporium on the 30th.

Least daily range,  $1^{\circ}$  at Pottstown on the 27th.

From January 1, 1891, to March 31, 1891, the excess in temperature at Philadelphia was  $185^{\circ}$ , at Erie  $261^{\circ}$ , and at Pittsburgh  $258^{\circ}$ .

The warmest days of the month were the 18th, 23d and 30th. The coldest were the 1st and 2d.

### BAROMETER.

The mean pressure for the month,  $30^{\circ}\cdot 10$ , is about  $\cdot 08$  above the normal. At the U. S. Signal Service Stations, the highest observed was  $30^{\circ}\cdot 63$ , at Pittsburgh, on the 1st, and the lowest,  $29^{\circ}\cdot 36$ , at Pittsburgh, on the 13th.

### PRECIPITATION.

The average precipitation,  $5^{\circ}\cdot 10$  inches for the month, is an excess of about  $2^{\circ}\cdot 00$  inches. The largest totals in inches (including melted snow) were Selins Grove,  $8^{\circ}\cdot 39$ ; West Chester,  $8^{\circ}\cdot 33$ ; Kennett Square,  $8^{\circ}\cdot 04$ ; Charlesville,  $7^{\circ}\cdot 89$ ; McConnellsburg,  $7^{\circ}\cdot 45$ , and Phoenixville,  $7^{\circ}\cdot 42$ .

The least were Erie,  $2^{\circ}\cdot 44$ , and Columbus,  $2^{\circ}\cdot 72$ .

The largest monthly snowfall totals in inches were Charlesville, 39; Blue Knob, 34; Carlisle, 33, and Grampian Hills, 31.

The snow storm of the 27th was very severe in the Schuylkill and Lebanon valleys. Heavy drifts occurred and caused delay to traffic. The snow soon melted and very little was left on the ground at the end of the month.

#### WIND AND WEATHER.

The prevailing wind was from the Northeast. The month was cold, wet, and generally unpleasant and unhealthy. Vegetation is backward. The prospects for grain and fruit are favorable.

Average number: Rainy days, 14; clear days, 7; fair days, 8; cloudy days, 16.

#### MISCELLANEOUS PHENOMENA.

*Thunder-storms.*—Quakertown, 9th; Phœnixville, 9th; Carlisle, 9th; Coopersburg, 9th; Wilkes-Barre, 9th; Pottstown, 9th; Philadelphia, 9th.

*Hail.*—Blue Knob, 20th; Le Roy, 20th; Mauch Chunk, 15th, 20th; Phœnixville, 20th; Lock Haven, 14th; Carlisle, 20th; Swarthmore, 9th; Wellsboro, 20th; Lewisburg, 13th, 20th.

*Snow.*—Charlesville, 2d, 3d, 4th, 6th, 13th, 14th, 20th, 27th, 28th; Blue Knob, 2d, 3d, 4th, 6th, 7th, 9th, 10th, 13th, 14th, 15th, 16th, 20th, 22d, 27th, 28th, 31st; Hollidaysburg, 2d, 3d, 4th, 6th, 9th, 13th, 14th, 20th, 22d, 27th, 28th; Wysox, 2d, 3d, 6th, 20th, 31st; Le Roy, 3d, 4th, 6th, 16th, 20th, 21st, 27th; Quakertown, 3d, 4th, 6th, 14th, 21st, 31st; Johnstown, 2d, 3d, 4th, 6th, 9th, 13th, 16th, 20th, 27th, 28th; Emporium, 4th, 20th, 27th, 28th; State College, 2d, 6th, 9th, 13th, 14th, 20th, 21st, 27th, 28th, 31st; Coatesville, 1st, 3d, 6th, 14th, 20th, 28th, 29th; Kennett Square, 3d, 20th, 27th, 28th; Phœnixville, 3d, 6th, 7th, 14th, 31st; Westtown, 2d, 3d, 6th, 20th, 27th, 28th; Rimersburg, 2d, 3d, 4th, 6th, 9th, 14th, 22d, 27th, 28th; Grampian Hills, 3d, 4th, 6th, 9th, 13th, 14th, 20th, 27th, 31st; Meadville, 3d, 4th, 13th, 14th, 27th, 28th; Carlisle, 6th, 27th, 28th; Swarthmore, 3d, 28th; Uniontown, 2d, 9th, 13th, 14th, 27th, 28th; Huntingdon, 2d, 3d, 4th, 27th, 28th; Indiana, 2d, 3d, 4th, 6th, 13th, 16th, 27th, 28th; Lancaster, 3d, 4th, 27th, 28th; New Castle, 2d, 4th, 9th, 14th, 27th, 28th; Lebanon, 3d, 4th, 7th, 20th, 27th, 28th; Coopersburg, 3d, 4th, 6th, 21st, 31st; Wilkes-Barre, 4th, 20th; Smethport, 2d, 3d, 4th, 6th, 13th, 14th, 20th, 27th; Greenville, 1st, 2d, 3d, 4th, 5th, 6th, 9th, 13th, 14th, 15th, 16th, 19th, 27th, 28th; Pottstown, 3d, 27th; Philadelphia, 3d, 4th, 6th, 7th, 14th, 27th, 28th; Girardville, 2d, 3d, 4th, 20th, 27th, 31st; Selins Grove, 2d, 3d, 4th, 27th, 28th; Somerset, 3d, 4th, 7th, 27th, 28th; Eagles Mere, 4th, 5th, 6th, 7th, 13th, 14th, 20th, 21st, 27th, 31st; Lewisburg, 3d, 6th, 20th, 27th, 31st; Dyberry, 3d, 4th, 5th, 7th, 14th, 16th, 19th, 29th; Honesdale, 3d, 4th, 31st; Ligonier, 2d, 3d, 14th, 27th, 28th; South Eaton, 3d, 4th, 20th, 21st; York, 3d, 4th, 7th, 14th, 21st, 27th, 28th.

*Frost.*—Charlesville, 24th; Hollidaysburg, 6th, 11th, 17th, 18th, 26th, 30th; Quakertown, 10th, 11th, 30th; State College, 6th, 11th, 17th, 26th, 30th; Phœnixville, 10th, 15th, 19th, 30th; Westtown, 11th; Grampian Hills, 6th; Meadville, 30th; Lancaster, 11th; New Castle, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 9th, 10th, 11th, 13th, 14th, 15th, 16th, 17th, 18th, 19th, 24th, 25th, 26th, 27th,

28th, 29th, 30th; Lebanon, 11th; Coopersburg, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 10th, 11th, 14th, 15th, 16th, 17th, 20th, 26th, 27th; Nisbet, 1st, 2d, 6th, 11th, 30th; Selins Grove, 10th, 11th; Eagles Mere, 6th, 17th; Columbus, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 10th, 11th, 14th, 15th, 16th, 17th, 18th, 19th, 21st, 24th, 25th, 26th, 27th, 28th, 29th, 30th; South Eaton, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 10th, 11th, 14th, 15th, 16th, 17th, 18th, 19th, 20th, 21st, 26th, 27th; York, 11th.

*Sleet*.—Charlesville, 9th, 20th; Blue Knob, 8th, 9th, 20th, 21st, 22d, 31st; Hollidaysburg, 31st; Quakertown, 20th; Mauch Chunk, 20th; Grampian Hills, 8th; Lock Haven, 20th, 21st; Uniontown, 6th; Coopersburg, 21st; Greenville, 27th; Philadelphia, 20th, 27th; Dyberry, 9th, 20th, 21st; Honesdale, 20th; South Eaton, 20th; York, 13th, 20th.

*Aurora*.—Coopersburg, 26th; Eagles Mere, 16th, 30th; Dyberry, 30th.

*Coronæ*.—Charlesville, 15th, 24th, 25th; Le Roy, 7th; Rimersburg, 25th; Lebanon, 2d, 11th, 15th, 16th, 17th, 18th, 25th, 26th; Dyberry, 18th, 26th.

*Solar Halos*.—Blue Knob, 30th; Le Roy, 2d, 6th, 7th, 11th, 18th, 25th, 26th; State College, 6th; Grampian Hills, 11th; Meadville, 30th; Huntingdon, 1st, 26th, 30th; Lebanon, 2d, 6th, 11th, 26th; Philadelphia, 2d, 6th, 11th, 25th, 26th, 30th; Eagles Mere, 2d, 6th, 11th, 18th, 25th, 26th; Dyberry, 2d, 6th, 18th, 26th; York, 15th.

*Lunar Halos*.—Charlesville, 25th; Hollidaysburg, 15th; Le Roy, 26th; Quakertown, 26th; State College, 15th; Phoenixville, 15th, 26th; Rimersburg, 24th; Meadville, 18th; Huntingdon, 15th; Lancaster, 18th; Lebanon, 18th, 26th; Coopersburg, 26th; Greenville, 17th; Eagles Mere, 18th, 26th; Philadelphia, 15th, 16th; Dyberry, 18th; South Eaton, 18th; York, 15th.

*Polar Bands*.—Le Roy, 17th.

*Parhelias*.—Le Roy, 6th, 11th, 30th; Eagles Mere, 6th; Dyberry, 17th, 27th.

*Zodiacal Lights*.—Le Roy, 10th; Coatesville, 4th, 5th, 10th, 13th.

#### WEATHER FORECASTS.

Percentage of local verifications of weather and temperature signals as reported by displaymen for March, 1891:

Weather, 90 per cent.

Temperature, 90 per cent.

#### TEMPERATURE AND WEATHER SIGNAL DISPLAY STATIONS.

<i>Displayman.</i>	<i>Station.</i>
U. S. Signal Office, . . . . .	Philadelphia.
Wanamaker & Brown, . . . . .	"
Pennsylvania Railroad Company, . . . . .	"
Continental Brewing Company, . . . . .	"
Samuel Simpson, . . . . .	"
B. T. Baubitt, . . . . .	"
Western Meat Company, . . . . .	"
Neptune Laundry, . . . . .	"



<i>Displayman.</i>	<i>Station.</i>
C. W. Burkhardt, . . . . .	Shoemakersville.
A. N. Lindenmuth, . . . . .	Allentown.
C. B. Whitehead, . . . . .	Bradford.
Thomas F. Sloan, . . . . .	McConnellsburg.
J. H. Fulmer, . . . . .	Muncy.
W. T. Butz, . . . . .	New Castle.
Capt. A. Goldsmith, . . . . .	Quakertown.
Frank Ross, . . . . .	Oil City.
Lerch & Rice, . . . . .	Bethlehem.
Signal Office, . . . . .	Erie.
J. R. Raynsford, . . . . .	Montrose.
E. P. Wilbur & Co., . . . . .	South Bethlehem.
Agricultural Experiment Station, . . . . .	State College.
Signal Office, . . . . .	Pittsburgh.
<i>New Era</i> , . . . . .	Lancaster.
D. G. Hurley, . . . . .	Altoona.
J. E. Forsythe, . . . . .	Butler.
Steward M. Dreher, . . . . .	Stroudsburg.
State Normal School, . . . . .	Millersville.
E. C. Wagner, . . . . .	Girardville.
L. H. Grenewald, . . . . .	York.
J. E. Pague, . . . . .	Carlisle.
C. L. Peck, . . . . .	Coudersport.
H. D. Miller, . . . . .	Drifton.
M. Tannehill, . . . . .	Confluence.
S. C. Burkholder, . . . . .	Pollock.
Robt. M. Graham, . . . . .	Catawissa.
Henry F. Bitner, . . . . .	Millersville.
A. M. Wildman, . . . . .	Langhorne.
G. W. Klee, . . . . .	Chambersburg.
A. Simon's Sons, . . . . .	Lock Haven.
<i>Raftsmen's Journal</i> , . . . . .	Clearfield.
R. C. Schmidt & Co., . . . . .	Belle Vernon.
Chas. B. Lutz, . . . . .	Bloomsburg.
E. C. Lorentz, . . . . .	Johnstown.
W. M. James, . . . . .	Ashland.
Miller & Allison, . . . . .	Punxsutawney.
E. J. Sellers, . . . . .	Kutztown.
H. M. Kaisinger, . . . . .	Hartsville.
Foult & Co., . . . . .	Milford.
William Lawton, . . . . .	Wilmington, Del.
Wister Heberton & Co., . . . . .	Germantown.
Charles M. Mullen, . . . . .	Bedford.
E. W. Merrill, . . . . .	North East.
A. Simon's Sons, . . . . .	Lock Haven.
Frank Ridgway, . . . . .	Harrisburg.

<i>Displayman.</i>	<i>Station.</i>
G. W. Yost, . . . . .	Collegeville.
A. H. Gerbench, . . . . .	Annaville.
Knowles Croskey, . . . . .	Phoenixville.
Powell Bros., . . . . .	Shadeland.
Lincoln E. Rowley, . . . . .	Athens.
P. S. Weber, . . . . .	Du Bois.
E. Jennet, . . . . .	Franklin.
Milton C. Cooper, . . . . .	Ashbourne.
J. L. Long, . . . . .	Freeport.
J. H. Apple, . . . . .	Clarion.
George Wheeler, . . . . .	Doylestown.
J. C. McCloskey, . . . . .	Lock Haven.
Charles Petersen, . . . . .	Honesdale.
Armstrong & Brownell, . . . . .	Smethport.
Frank Loomis, . . . . .	Troy.



# STATE WEATHE

	Relative Humidity.	Dew Point.	Total Inches.
Alle	71.6	27.8	3.11
Bedf	75.0	25.5	7.89
Berk	...	...	5.35
Blair	75.5	29.0	2.62
Blair	...	...	5.56
Blair	75.0	26.5	5.60
Brad	74.5	24.7	3.12
Brad	...	...	3.15
Buck	...	...	4.61
Buck	77.0	27.1	5.59
Cam	74.0	27.0	4.99
Cam	...	...	5.12
Carb	...	...	5.69
Cen	84.6	27.3	4.07
Ches	...	...	8.33
Ches	...	...	7.61
Ches	...	...	8.04
Ches	82.6	29.7	7.42
Ches	...	...	5.38
Clari	...	...	...
Clari	...	...	...
Clea	...	...	3.97
Clint	...	...	4.06
Colu	85.0	...	...
Craw	83.0	26.6	3.37
Cum	...	28.0	6.52
Daup	71.4	25.0	4.25
Dela	8.0	...	...
Erie	81.0	30.0	5.78
Faye	...	25.0	2.44
Fran	...	...	4.22
Fult	82.0	29.3	7.45
Hun	...	...	4.48
Hun	...	...	...
Indi	81.9	29.3	3.50
Land	81.5	34.1	6.30
Law	84.7	35.4	3.32
Leba	75.7	27.7	5.30
Lehi	...	...	6.54
Luze	...	...	...
Luze	...	...	3.67
Lyce	...	...	5.20
McK	...	...	...
Mer	92.6	28.0	3.33
Mim	74.5	28.5	6.60
Nort	...	...	...
Phil	70.0	26.0	4.42
Phil	78.0	30.1	5.76
Schu	81.2	...	3.80
Snye	29.2	...	8.39
Som	92.1	31.1	5.37
Sulli	72.6	20.1	5.88
Tiog	75.4	38.2	2.72
Unio	...	...	6.40
War	...	...	2.72
Was	...	...	...
Way	...	...	4.19
Way	...	...	4.32
Ves	...	...	4.50
Vyc	...	...	4.54
Worl	86.4	29.6	6.07

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## MONTHLY SUMMARY OF REPORTS BY VOLUNTARY OBSERVERS OF THE PENNSYLVANIA STATE WEATHER SERVICE FOR MARCH, 1891.

COUNTY.	STATION.	Elevation above Sea Level (feet).	BAROMETER REDUCED TO SEA LEVEL.			TEMPERATURE.						DAILY RANGE.					RELATIVE HUMIDITY.		PRECIPITATION.		NUMBER OF DAYS.			WIND.			OBSERVERS.					
			Mean.	Highest.	Lowest.	Mean.	Highest.	Date.	Lowest.	Date.	Mean of Maximum.	Mean of Minimum.	Mean.	Greatest.	Date.	Least.	Date.	Relative Humidity.	Dew Point.	Total Inches.	Total Snowfall During Month.	Depth of Snow on Ground at End of Month.	Number of Days Rainfall.	Clear.	Fair.	Cloudy.		P. A. M.	P. M.	P. M.		
																															MAXIMUM.	MINIMUM.
Allegheny.	Pittsburgh.	847	30°100	30°530	29°360	37°6	56	18	11	1	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	3.12	7°60	...	17	6	7	18	SE	SE	SE	Oscar D. Stewart, Sgt. Sig. Corps.		
Bedford.	Reading.	1,300	...	...	...	37°8	57	18	1	3	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	2.89	29°00	...	15	7	3	21	SW	SW	SE	Miss A. G. Apple.		
Berks.	Charlestown.	280	...	...	...	38°6	66	18	1	1	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	2.53	3°37	...	16	...	...	...	...	...	...	Franklin Vager.		
Blanco.	Altoona.	1,181	...	...	...	38°2	66	18	1	1	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	2.55	34°00	24°00	19	5	0	17	SE	SE	SE	Charles B. Dudley.		
Blair.	Blue Knob.	2,500	...	...	...	38°2	66	18	1	1	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	2.55	34°00	24°00	19	5	0	17	SE	SE	SE	A. H. Boyle.		
Blaine.	Harrisburg.	2,500	...	...	...	38°2	66	18	1	1	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	2.55	34°00	24°00	19	5	0	17	SE	SE	SE	Prof. J. A. Stewart.		
Bradford.	Wysox.	718	30°130	30°590	29°400	33°7	61	18	1	1	44°5	29°8	14°7	30°0	17	6°0	4	71°5	27°8	2.52	13°00	...	11	8	0	16	SE	SE	SE	Charles Beecher.		
Butler.	The Roy.	1,400	...	...	...	39°9	52	30	2	2	37°3	23°0	13°7	27°0	17	2°0	31	...	...	3.15	12°00	...	15	8	0	15	NW	NE	NE	Geo. W. Warburton.		
Bucks.	Fort of Nesheim.	1,400	...	...	...	39°9	52	30	2	2	37°3	23°0	13°7	27°0	17	2°0	31	...	...	3.15	12°00	...	15	8	0	15	NW	NE	NE	C. Hildman.		
Camden.	Quakertown.	335	30°120	30°600	29°670	34.4	63	23	5	2	43°8	25°5	18°3	35°0	30	7°2	21	70°7	27°1	2.59	3°75	...	12	5	12	14	NE	NE	NE	J. L. Heacock.		
Carbon.	Johnstown.	1,414	30°150	30°660	29°490	35°0	61	20	6	1	43°7	27°3	15°9	38°0	18	2°0	8	71°0	27°0	4.99	17°00	...	11	7	15	15	SE	SE	SE	W. Laurent.		
Centre.	Kempston (38 days).	1,630	...	...	...	37°7	60	18	3	4	43°3	23°0	13°7	27°0	17	2°0	31	...	...	3.69	...	...	11	7	8	15	NW	E	E	T. B. Lloyd.		
Centre.	Mauch Chunk (38 days).	550	...	...	...	34°9	...	...	...	...	43°3	23°0	13°7	27°0	17	2°0	31	...	...	3.69	...	...	8	11	5	22	NW	NW	NW	John J. Boyd.		
Chesapeake.	State College Experiment Station.	1,191	30°077	30°588	30°381	31°6	54	28	30	0	2	38°6	24°2	14°4	30°0	30	2°0	8	84°6	27°3	4.07	15°00	...	14	4	12	15	E	E	E	Prof. Wm. Freat.	
Chesapeake.	West Chester.	435	30°094	30°523	29°432	35°0	61	23	10	2	43°8	28°5	14°3	27°0	18	4°5	27	...	...	3.33	18°00	...	15	13	5	15	NW	NW	NW	Jesse C. Green, D.D.S.		
Chesapeake.	Coatesville.	380	...	...	...	34°5	66	23	10	2	43°5	27°1	16°4	31°0	11	3°0	3	...	...	3.04	25°00	...	15	0	9	15	NE	NE	NE	W. T. Gordon.		
Chesapeake.	Kennett Square.	275	...	...	...	34°5	66	23	10	2	43°5	27°1	16°4	31°0	11	3°0	3	...	...	3.04	25°00	...	17	0	8	14	NE	NE	NE	Benj. F. Kirk.		
Chesapeake.	Phoenixville.	304	30°124	30°570	29°566	35°0	61	20	6	1	43°7	27°3	15°9	38°0	18	2°0	8	71°0	27°0	4.99	17°00	...	11	7	15	15	SE	SE	SE	Knott's Creek.		
Chesapeake.	Westtown.	350	30°140	30°585	29°548	36°0	62	23	11	2	43°7	30°1	13°6	25°0	11	2°5	27	...	...	3.38	13°00	...	12	0	9	13	N	E	E	Prof. Wm. F. Wickersham.		
Chesapeake.	Rimersburg.	1,500	...	...	...	31°7	60	18	2	1	43°7	24°3	14°6	...	...	...	...	...	...	3.50	...	...	16	5	9	17	E	E	E	Rev. W. W. Detrick, A.M.		
Chesapeake.	Clarion.	1,530	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	C. M. Thomas, B.S.	
Chesapeake.	State Normal School.	1,530	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Nathan Moore.	
Cleaveland.	Grampian Hills.	1,430	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Prof. John A. Robb.	
Columbia.	Lock Haven.	1,430	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Robert M. Graham.	
Columbia.	Catawissa.	491	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	J. & H. H. Metcalf.
Crawford.	Meadville.	1,390	30°058	30°660	...	34°4	66	18	1	1	44°0	29°0	17°2	31°0	11	3°0	20	...	...	3.50	26°0	...	10	1	10	18	NW	SE	SE	J. E. Pague.		
Cumbersburg.	Carlisle.	480	...	...	...	33°8	60	20	10	6	41°8	27°1	14°7	32°0	30	3°0	8	81°0	28°0	0.54	33°00	...	17	0	7	15	NE	N	N	Frank Ridgway, Sgt. Sig. Corps.		
Dauphin.	Harrisburg.	301	30°124	30°621	30°600	34°0	53	19, 21, 26	10	2	41°1	38°8	22°0	26°0	14	3°0	8	71°4	25°0	4.93	22°00	...	15	11	4	10	NW	NE	NE	...		
Delaware.	Swarthmore College.	390	30°087	30°432	30°511	36°8	66	23	17	17	44°0	29°0	17°2	31°0	11	3°0	27	81°0	30°0	3.78	5°00	...	10	3	11	18	NE	NE	NE	Prof. Susan J. Cunningham.		
Delaware.	Grampian Hills.	1,430	...	...	...	34°4	66	18	1	1	44°0	29°0	17°2	31°0	11	3°0	27	81°0	30°0	3.78	5°00	...	10	3	11	18	NE	NE	NE	Peter Wood, Sgt. Sig. Corps.		
Delaware.	Uniontown.	1,000	30°093	30°512	30°538	36°0	65	18	2	1	44°5	29°5	17°0	34°0	18	5°0	23	...	...	4.22	3°00	...	12	8	13	10	NW	W	NW	Wm. Hunt.		
Delaware.	Chambersburg.	618	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Miss Mary A. Ricker.	
Fulton.	Wilson Female College.	875	...	...	...	34.4	58	25	7	6	43°5	26°3	17°2	34°0	30	5°0	8	81°0	29°3	7.45	24°00	4°00	13	9	5	17	N	NW	NW	Thomas F. Sowell.		
Huntingdon.	Huntingdon.	650	...	...	...	32°5	60	18, 19, 30	5	1	4	40°0	23°0	19°0	30°0	18	5°0	8	...	...	4.48	...	...	5	8	10	13	W	W	W	Prof. W. J. Swigart.	
Huntingdon.	The Normal College.	700	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	J. E. Rooney.	
Indiana.	Petersburg.	700	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Prof. S. C. Schumaker.	
Indiana.	State Normal School (30 days).	1,350	30°048	30°093	29°547	34°1	63	18	5	1	4°3	23°0	19°3	37°0	18	5°0	20	81°0	29°3	3°50	4°05	...	16	4	11	15	N	SE	SW	Prof. S. C. Schumaker.		
Lancaster.	Lancaster.	413	30°139	30°043	30°257	35°5	57	23	10	3	45°3	25°2	20°4	33°5	18	8°0	21	51°5	34°1	6°30	21°00	...	13	5	13	13	N	NW	E	Levin T. Lampe.		
Lawrence.	Franklin and Marshall College.	413	30°139	30°043	30°257	35°5	57	23	10	3	45°3	25°2	20°4	33°5	18	8°0	21	51°5	34°1	6°30	21°00	...	13	5	13	13	N	NW	E	Wm. T. Hunt.		
Lebanon.	New Castle.	932	...	...	...	36°4	64	18	2	1	45°9	24°5	18°4	33°0	30	6°0	22	34°7	35°4	3°30	8°25	...	11	6	12	13	NE	SE	SE	Geo. W. Hayes, C.E.		
Lebanon.	Lebanon.	474	30°077	30°586	29°440	35°7	62	20	8	2	44°4	27°4	15°8	27°0	30	6°0	8	71°7	27°7	5°30	21°00	...	15	6	12	13	NE	NW	NW	M. H. Boyce.		
Lehigh.	Coopersburg.	580	...	...	...	35°0	63	30	8	2	42°6	27°7	14°0	34°0	30	3°0	20	...	...	6.54	6°50	...	12	9	5	17	NW	NW	NW	...		
Luzerne.	Drifcon.	1,658	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	H. D. Miller, M.D.	
Luzerne.	Wilkes-Barre.	575	...	...	...	36°1	61	30	5	2	45°6	26°0	19°0	30°0	14	10°0	21	...	...	6.17	10°00	...	18	1	11	16	N	NE	NE	A. W. Bettley.		
Lycoming.	Niobe.	550	...	...	...	34°8	61	18	1	1	44°0	21°2	18°0	38°0	30	6°0	27	...	...	5.20	20°00	...	11	8	6	17	S	S	S	John S. Gibson, P.M.		
McKean.	McKean.	1,800	...	...	...	31°9	61	18	1	1	44°0	21°2	18°0	38°0	30	6°0	27	...	...	5.20	20°00	...	11	8	6	17	S	S	S	Arneston S. Russell.		
Mercer.	Greenville.	1,000	30°045	30°377	30°335	37°0	61	18	0	1	41°8	24°2	17°6	30°5	18	4°4	14	71°6	28°0	3.33	18°00	...	15	4	11	16	N	NW	NW	Prof. S. B. Miller.		
Mifflin.	Thief College.	150	...	...	...	36°0	61	30	10	2	43°0	30°0	13°0	27°0	11	10	21	...	...	6.60	8°00	...	10	11	6	12	W	NW	NW	Charles Moore, D.D.S.		
Monroe.	Behlheim.	360	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	Lorch & Rice.	
Philadelphia.	Philadelphia.	117	30°109	30°580	30°540	38°0	61																									



Meadville.	McConnellsburg.	New Castle.	Nisbet.
..	..	'04	..
'20	'20	..	..
'35	'30	'08	'60
..	'05	..	..
..	..	..	..
'56	'70	..	'20
..	'35	'18	'60
..	..	..	..
'34	'76	'75	'40
'56	'22	'19	'70
'10	..	'02	..
..	..	..	..
..	..	..	..
..	..	..	..
..	'02	..	..
'49	'54	'39	'60
..	..	..	..
..	'50	'08	'20
..	'30	..	'30
..	..	..	'10
..	..	..	..
'10	'32	..	..
'20	'88	'86	'00
..	..	..	..
..	'33	..	..
'47	..	'71	'50
3'37	7'45	3'32	5'20

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## PRECIPITATION FOR MARCH, 1891.

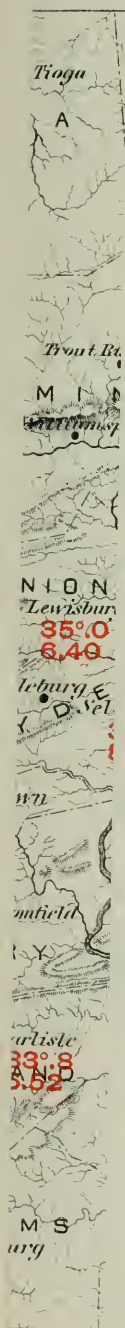
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\* No record.

† U. S. Signal Office.

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# PENNSYLVANIA STATE WEATHER SERVICE.

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## MONTHLY WEATHER REVIEW.

FOR APRIL, 1891.

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*Prepared under the Direction of the Committee on Meteorology of the  
Franklin Institute.*

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, April 30, 1891.

### TEMPERATURE.

The mean temperature of 50 stations for April, 1891, was  $49^{\circ}8$ , which is about  $3^{\circ}0$  above the normal, and  $1^{\circ}1$  above the corresponding month of 1890.

The mean of the daily maxima and minima temperatures  $61^{\circ}0$  and  $38^{\circ}5$  give an average daily range of  $22^{\circ}5$ , and a monthly mean of  $49^{\circ}7$ .

Highest monthly mean,  $54^{\circ}2$  at Philadelphia, S. O.

Lowest monthly mean,  $44^{\circ}4$  at Dyberry.

Highest temperature recorded during the month,  $87^{\circ}0$  on the 30th at Carlisle, and on the 27th at Lewisburg.

Lowest temperature  $11^{\circ}4$  on the 5th at Greenville.

Greatest local monthly range,  $68^{\circ}$  at Uniontown and Greenville.

Least local monthly range,  $53^{\circ}$  at Philadelphia.

Greatest daily range,  $55^{\circ}$  at Columbus on the 17th.

Least daily range,  $1^{\circ}$  at Le Roy on the 4th.

From January 1, 1891, to April 30, 1891, the excess in temperature at Philadelphia was  $286^{\circ}$ , at Erie  $347^{\circ}$ , and at Pittsburgh  $332^{\circ}$ .

The warmest days of the month were the 18th, 19th, 20th, 21st, 22d, 27th and 30th. The coldest were the 4th, 5th, 6th, 7th and 8th.

### BAROMETER.

The mean pressure for the month,  $30^{\circ}02$ , is about  $\cdot 02$  above the normal. At the U. S. Signal Service Stations, the highest observed was  $30^{\circ}54$ , at Philadelphia, on the 10th, and the lowest,  $29^{\circ}48$ , at Philadelphia, on the 3d.

### PRECIPITATION.

The average precipitation,  $2^{\circ}08$  inches for the month, is a deficiency of about three-quarters of an inch. The largest totals in inches (including melted snow) were Girardville,  $3^{\circ}54$ ; Eagles Mere,  $3^{\circ}15$ ; Philadelphia,  $2^{\circ}82$ ; Quakertown,  $2^{\circ}74$ , and Johnstown,  $2^{\circ}73$ .

The least were Wellsboro,  $1^{\circ}12$ ; Somerset,  $1^{\circ}13$ ; Carlisle,  $1^{\circ}18$ , and Pittsburgh,  $1^{\circ}18$ .

The largest monthly snowfall totals in inches were Blue Knob, 16; Dyberry, 10; Eagles Mere, 5, and Le Roy, 5.



No snow reported on ground at the end of the month.

#### WIND AND WEATHER.

The prevailing wind was from the Northwest. The month was seasonable, with a slight excess of temperature, and a deficiency of rainfall. The prospects for grain, grass and fruit continue favorable.

Average number: Rainy days, 8; clear days, 13; fair days, 9; cloudy days, 8.

#### MISCELLANEOUS PHENOMENA.

*Thunder-storms.*—Blue Knob, 14th, 18th, 22d; Hollidaysburg, 22d; Le Roy, 16th, 18th, 22d; Quakertown, 6th, 22d; Johnstown, 18th, 22d; Emporium, 18th, 22d; State College, 18th, 22d; Coatesville, 18th; Kennett Square, 18th; Phoenixville, 16th, 18th; Rimersburg, 14th, 18th, 22d, 30th; Meadville, 11th, 14th, 22d, 30th; Carlisle, 11th, 18th, 22d; Harrisburg, 11th, 18th; Swarthmore, 16th, 18th; Uniontown, 18th; Huntingdon, 18th, 22d; Lancaster, 11th, 18th, 22d; Lebanon, 11th, 14th, 18th, 22d; Coopersburg, 16th, 22d; Wilkes-Barre, 11th, 16th, 22d; Nisbet, 18th, 22d; Smethport, 30th; Pottstown, 16th; Philadelphia, 15th, 16th, 18th, 22d; Girardville, 10th, 12th, 18th, 22d; Selins Grove, 18th, 23d; Eagles Mere, 16th, 18th, 22d; Wellsboro, 18th, 22d; Lewisburg, 18th; Columbus, 22d, 30th; Dyberry, 18th, 22d; Ligonier, 18th; South Eaton, 18th, 22d, 23d; York, 11th, 18th.

*Hail.*—Le Roy, 16th; Johnstown, 22d; Coopersburg, 16th; Greenville, 30th; Philadelphia, 16th; Dyberry, 18th; Honesdale, 18th; South Eaton, 23d; York, 11th.

*Snow.*—Blue Knob, 2d, 3d, 4th, 5th, 6th, 7th, 12th; Hollidaysburg, 2d, 4th; Le Roy, 1st, 2d, 3d; Johnstown, 3d, 4th; Coatesville, 5th, 6th, 7th; Rimersburg, 2d, 3d, 4th; Meadville, 4th; Uniontown, 4th, 5th, 6th, 7th; New Castle, 4th, 6th; Lebanon, 4th; Wilkes-Barre, 2d; Greenville, 4th, 6th; Philadelphia, 6th, 7th; Selins Grove, 4th; Somerset, 2d, 4th; Eagles Mere, 2d, 3d, 7th, 10th; Lewisburg, 3d; Dyberry, 1st, 2d, 3d, 4th, 7th, 10th, 25th.

*Frost.*—Charlesville, 24th, 25th, 28th; Blue Knob, 13th, 17th, 25th, 26th, 29th; Hollidaysburg, 7th, 8th, 9th, 13th, 25th, 26th; Quakertown, 6th, 8th, 9th, 13th, 21st, 25th, 26th; Emporium, 5th, 7th, 8th, 9th, 25th, 26th, 29th; State College, 6th, 7th, 8th, 9th, 13th, 21st; West Chester, 25th; Coatesville, 5th, 6th, 7th, 8th, 9th, 13th, 25th, 26th; Phoenixville, 6th, 7th, 8th, 9th, 25th, 26th; Rimersburg, 13th, 17th, 25th, 26th, 29th; Grampian Hills, 7th, 9th, 13th; Carlisle, 6th, 7th, 9th; Uniontown, 7th, 8th, 9th, 25th, 26th, 29th, 30th; Huntingdon, 24th, 26th; Lancaster, 6th, 8th, 19th; New Castle, 3d, 4th, 5th, 6th, 7th, 8th, 13th, 25th, 26th, 29th; Lebanon, 7th, 8th, 9th; Coopersburg, 5th, 6th, 7th, 8th, 9th; Wilkes-Barre, 5th, 8th, 27th; Nisbet, 8th, 9th, 25th, 26th; Greenville, 7th, 8th, 9th, 13th, 21st, 25th, 26th, 29th; Philadelphia, 5th, 6th, 7th, 8th, 9th; Girardville, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 11th, 25th, 26th, 29th; Selins Grove, 6th, 8th, 13th, 24th, 25th; Somerset, 13th, 27th, 28th; Eagles Mere, 8th, 9th, 16th, 17th, 21st, 22d; Wellsboro, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 21st, 25th, 26th, 29th; Columbus, 4th,

5th, 6th, 7th, 8th, 9th, 13th, 17th, 25th, 26th, 29th; Dyberry, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 11th, 17th, 21st, 25th, 26th, 29th, 30th; Honesdale, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 25th, 26th, 29th, 30th; South Eaton, 1st, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 9th, 10th, 25th, 26th, 29th; York, 5th, 6th, 7th, 8th, 9th, 13th, 25th, 26th.

*Sleet*.—Blue Knob, 12th; Uniontown, 3d.

*Aurora*.—Greenville, 8th, 27th, 30th; Eagles Mere, 7th, 8th.

*Coronæ*.—Le Roy, 13th; Quakertown, 13th, 17th; Rimersburg, 13th, 19th; Grampian Hills, 7th; Lebanon, 13th, 15th, 16th, 17th, 19th, 20th, 21st, 22d, 28th; Greenville, 19th, 20th; Philadelphia, 13th, 15th, 17th; Eagles Mere, 13th; Dyberry, 13th, 17th; York, 26th.

*Solar Halos*.—Le Roy, 14th, 29th; Philadelphia, 5th, 6th, 14th, 24th, 29th; Eagles Mere, 9th, 29th.

*Lunar Halos*.—Charlesville, 9th; Hollidaysburg, 17th, 18th; State College, 13th, 16th, 18th, 21st; Phoenixville, 17th, 22d; Meadville, 12th, 19th, 20th; Carlisle, 13th, 14th; Huntingdon, 18th, 20th; Greenville, 18th; Eagles Mere, 22d.

*Meteors*.—Rimersburg, 6th; Greenville, 19th.

*Zodiacal Lights*.—Coatesville, 5th, 6th, 7th, 8th.

#### WEATHER FORECASTS.

Percentage of local verifications of weather and temperature signals as reported by displaymen for April, 1891:

Weather, 84 per cent.

Temperature, 88 per cent.

#### TEMPERATURE AND WEATHER SIGNAL DISPLAY STATIONS.

<i>Displayman.</i>	<i>Station.</i>
U. S. Signal Office, . . . . .	Philadelphia.
Wanamaker & Brown, . . . . .	"
Pennsylvania Railroad Company, . . . . .	"
Continental Brewing Company, . . . . .	"
Samuel Simpson, . . . . .	"
B. T. Baobitt, . . . . .	"
Western Meat Company, . . . . .	"
Neptune Laundry, . . . . .	"
C. W. Burkhardt, . . . . .	Shoemakersville.
A. N. Lindenmuth, . . . . .	Allentown.
C. B. Whitehead, . . . . .	Bradford.
Thomas F. Sloan, . . . . .	McConnellsburg.
J. H. Fulmer, . . . . .	Muncy.
W. T. Butz, . . . . .	New Castle.
Capt. A. Goldsmith, . . . . .	Quakertown.
Frank Ross, . . . . .	Oil City.
Lerch & Rice, . . . . .	Bethlehem.
Signal Office, . . . . .	Erie.
J. R. Raynsford, . . . . .	Montrose.
E. P. Wilbur & Co., . . . . .	South Bethlehem.

<i>Displayman.</i>	<i>Station.</i>
Agricultural Experiment Station, . . . . .	State College.
Signal Office, . . . . .	Pittsburgh.
<i>New Era</i> , . . . . .	Lancaster.
D. G. Hurley, . . . . .	Altoona.
J. E. Forsythe, . . . . .	Butler.
Steward M. Dreher, . . . . .	Stroudsburg.
State Normal School, . . . . .	Millersville.
E. C. Wagner, . . . . .	Girardville.
L. H. Grenewald, . . . . .	York.
J. E. Pague, . . . . .	Carlisle.
C. L. Peck, . . . . .	Coudersport.
H. D. Miller, . . . . .	Drifton.
M. Tannehill, . . . . .	Confluence.
S. C. Burkholder, . . . . .	Pollock.
Robt. M. Graham, . . . . .	Catawissa.
Henry F. Bitner, . . . . .	Millersville.
A. M. Wildman, . . . . .	Langhorne.
G. W. Klee, . . . . .	Chambersburg.
A. Simon's Sons, . . . . .	Lock Haven.
<i>Raftsmen's Journal</i> , . . . . .	Clearfield.
R. C. Schmidt & Co., . . . . .	Belle Vernon.
Chas. B. Lutz, . . . . .	Bloomsburg.
E. C. Lorentz, . . . . .	Johnstown.
W. M. James, . . . . .	Ashland.
E. J. Sellers, . . . . .	Kutztown.
H. M. Kaisinger, . . . . .	Hartsville.
Foulk & Co., . . . . .	Milford.
William Lawton, . . . . .	Wilmington, Del.
Wister Heberton & Co., . . . . .	Germantown.
Charles M. Mullen, . . . . .	Bedford.
E. W. Merrill, . . . . .	North East.
Frank Ridgway, . . . . .	Harrisburg.
G. W. Yost, . . . . .	Collegeville.
A. H. Gerbench, . . . . .	Annville.
Knowles Croskey, . . . . .	Phoenixville.
Powell Bros., . . . . .	Shadeland.
Lincoln E. Rowley, . . . . .	Athens.
P. S. Weber, . . . . .	Du Bois.
E. Jennet, . . . . .	Franklin.
Milton C. Cooper, . . . . .	Ashbourne.
J. L. Long, . . . . .	Freeport.
J. H. Apple, . . . . .	Clarion.
George Wheeler, . . . . .	Doylestown.
J. C. McCloskey, . . . . .	Lock Haven.
Charles Petersen, . . . . .	Honesdale.
Armstrong & Brownell, . . . . .	Smethport.
Frank Loomis, . . . . .	Troy.

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MONTHLY SUMMARY OF REPORTS BY VOLUNTARY OBSERVERS OF THE PENNSYLVANIA STATE WEATHER SERVICE FOR APRIL, 1891

COUNTY.	STATION.	Elevation above Sea Level (feet).	BAROMETER REDUCED TO SEA LEVEL.			TEMPERATURE.										DAILY RANGE.			RELATIVE HUMIDITY.	DEW POINT.	PRECIPITATION.			NUMBER OF DAYS.			WIND.			OBSERVERS.
			Mean.	Highest.	Lowest.	Mean.	Highest.	Date.	Lowest.	Date.	Mean of Maximum.	Mean of Minimum.	Mean.	Greatest.	Date.	Least.	Date.	Total Inches.			Total Snowfall During Month.	Depth of Snow on Ground at End of Month.	Number of Days Rainfall.	Clear.	Fair.	Cloudy.	7 A. M.	2 P. M.	9 P. M.	
Allegheny	Pittsburgh.	847	30.040	30.140	29.970	53.4	81	21	24	5	63.5	43.7	10.5	38.0	17	74	4	59.2	17.3	1.18	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Berks	Charlestown.	1,300				53.4	81	18			63.5	43.7	10.5				59.2	17.3	1.18	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.			
Berks	Hamburg.	380				51.2	84	27	24	6, 8, 9	53.4	41.1	24.3	43.0	27	61	10	59.2	17.3	1.18	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Blair	Altoona.	1,181				53.5	82	22			63.5	41.1	24.3	35.0	27	61	2	48.3	34.0	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Blair	Blue Knob.	2,900				47.3	84	18	19		64.7	35.5	28.5	46.0	13	40		66.0	30.0	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Blair	Hollidaysburg.	247				51.2	84	27	24		50.3	34.9	24.3	48.5	30	4.0	4	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Bradford	Waynes.	1,400	30.024	30.184	29.953	47.3	83	22	20	5, 6	64.7	35.5	28.5	46.0	13	40		66.0	30.0	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Butler	Forks of Schuylkill.	1,400				47.3	83	27	20	5, 6	64.7	35.5	28.5	46.0	13	40		66.0	30.0	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Bucks	Quakertown.	530	30.030	30.020	29.940	50.4	81	19	21	8	61.6	37.4	18.2	49.3	27	7.3	10	66.0	30.3	2.01	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Bucks	Johnstown.	1,184	30.065	30.020	29.940	51.6	81	19	21	5, 6	61.6	37.4	18.2	49.3	27	7.3	10	66.0	30.3	2.01	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Camden	Emortown.	1,030				47.9	83	22	17	6	61.9	31.9	28.5	40.0	27	9.0	2	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Carroll	Manch Chunk (16 days).	1,350				47.5	80	19	20	8	56.1	38.9	17.2	31.0	14	2.0		71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Centre	Agricultural Experiment Station.	1,191	30.075	30.044	29.977	51.5	82	22	20	5, 6	61.9	31.9	28.5	40.0	27	9.0	2	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Chester	Wheat Creek.	455	30.021	30.078	29.983	51.6	81	27	5	7	61.7	41.5	19.2	33.0	30	5.0	10	60.0	30.0	2.02	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Chester	Coatesville.	350				51.5	84	30	22	9	59.3	38.3	25.7	45.0	30	10		71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Chester	Kennett Square.	275				49.7	74	19	30		62.0	41.9	20.0	41.5	27	10.0	4	74.4	42.4	2.02	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Chester	Phoenixville.	350	30.031	30.027	29.974	50.1	84	19	28	6	62.0	41.9	20.0	41.5	27	10.0	4	74.4	42.4	2.02	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Chester	Westtown.	350				49.7	80	17	21	15	5	56.7	39.9	16.8	44.0	17	4.0	3	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Clarion	Rimersburg.	1,350				48.2	80	17	21	15	5	56.7	39.9	16.8	44.0	17	4.0	3	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Clarion	Clarion.	1,350				48.2	80	17	21	15	5	56.7	39.9	16.8	44.0	17	4.0	3	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Clearfield	State Normal School.	1,350				48.2	80	17	21	15	5	56.7	39.9	16.8	44.0	17	4.0	3	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Clinton	Grampian Hills.	1,450				47.9	80	22	27	44	5	57.0	36.8	20.2	38.0	13	4.0	2	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Columbia	Lock Haven.	1,500				47.9	80	22	27	44	5	57.0	36.8	20.2	38.0	13	4.0	2	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Crawford	Carlisle.	1,320	30.060	30.050	29.800	47.2	76	22	30	14	5	58.0	36.6	20.3	48.0	17	10	4	80.0	41.3	1.53	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Cumberland	Harrisburg.	401	30.028	30.062	29.959	51.4	80	24	30	24	5, 6	58.3	35.5	26.7	45.0	30	8	2	80.0	41.3	1.53	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Delaware	Swarthmore.	301	30.028	30.062	29.959	51.4	80	24	30	24	5, 6	58.3	35.5	26.7	45.0	30	8	2	80.0	41.3	1.53	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Erie	University College.	1,190	30.062	30.050	29.954	52.3	84	19	25	7	62.4	41.4	19.0	34.0	27	4.0	10	68.0	37.0	1.50	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Erie	Univ. of Erie.	681	30.000	30.040	29.900	47.0	78	30	24	6	55.0	30.0	16.0	32.0	17	4.0	3	77.0	39.0	1.34	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Fayette	Uniontown.	1,000	30.000	30.072	29.970	53.1	83	21	15	5	64.1	41.4	20.0	38.0	17	4.0	3	77.0	39.0	1.34	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Franklin	Chambersburg.	1,000				53.1	83	21	15	5	64.1	41.4	20.0	38.0	17	4.0	3	77.0	39.0	1.34	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Fulton	Wilson Female College.	618				51.7	86	30	24	5, 6, 8	64.3	39.2	25.6	31.0	30	9.0	12	67.1	39.6	2.58	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Huntingdon	McConnellsburg.	575				51.7	86	30	24	5, 6, 8	64.3	39.2	25.6	31.0	30	9.0	12	67.1	39.6	2.58	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Huntingdon	The Normal College.	650				50.2	86	27	22	6, 7, 8, 9	63.5	39.9	26.6	32.0	30	9.0	12	67.1	39.6	2.58	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Indiana	Indiana.	500				50.2	86	27	22	6, 7, 8, 9	63.5	39.9	26.6	32.0	30	9.0	12	67.1	39.6	2.58	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Lancaster	State Normal School.	1,350				50.2	86	27	22	6, 7, 8, 9	63.5	39.9	26.6	32.0	30	9.0	12	67.1	39.6	2.58	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Lancaster	Lancaster.	1,350				50.2	86	27	22	6, 7, 8, 9	63.5	39.9	26.6	32.0	30	9.0	12	67.1	39.6	2.58	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Lawrence	Franklin and Marshall College.	411	30.068	30.065	29.976	53.9	82	19	24	5	63.0	39.3	25.7	46.0	27	5.0	2	74.6	45.3	2.16	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Lebanon	New Castle.	474	30.035	30.067	29.945	52.1	84	19	24	5	62.0	41.4	20.0	38.0	17	4.0	3	77.0	39.0	1.34	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Lehigh	Lebanon.	474	30.035	30.067	29.945	52.1	84	19	24	5	62.0	41.4	20.0	38.0	17	4.0	3	77.0	39.0	1.34	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Luzerne	Coopersburg.	530				51.7	80	16	22	35	8	60.9	40.2	20.2	30.0	17	4.0	10	60.5	30.3	1.70	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Luzerne	Drifcon.	1,655				51.7	80	16	22	35	8	60.9	40.2	20.2	30.0	17	4.0	10	60.5	30.3	1.70	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Luzerne	Drifcon Hospital.	1,655				51.7	80	16	22	35	8	60.9	40.2	20.2	30.0	17	4.0	10	60.5	30.3	1.70	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Luzerne	Wilkes-Barre.	575				51.3	84	18	22	22	8	63.9	38.9	25.9	45.0	30	12.0		71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.	
Luzerne	Violet.	550				49.8	84	27	15	6	58.6	39.7	25.9	40.5	30	15.5	4	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
McKean	Smithport.	1,500				49.8	84	27	15	6	58.6	39.7	25.9	40.5	30	15.5	4	71.5	38.5	1.75	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Mercer	Gettysburg.	1,000	29.961	30.063	29.930	47.7	79	17	11	5	59.2	35.3	22.9	44.5	13	12	1	64.3	40.9	1.74	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Montgomery	Pottsville.	150				53.3	83	19	20	8	64.4	42.4	21.9	40.0	13	12	1	64.3	40.9	1.74	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Montgomery	Thiel College.	350				53.3	83	19	20	8	64.4	42.4	21.9	40.0	13	12	1	64.3	40.9	1.74	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Philadelphia	Philadelphia.	137	30.000	30.040	29.980	54.4	85	19	30	5	61.4	44.5	18.9	30.0	30	7.0	4	59.0	38.0	2.14	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Philadelphia	Signal Office.	130	30.017	30.048	29.983	54.4	85	19	30	5	61.4	44.5	18.9	30.0	30	7.0	4	59.0	38.0	2.14	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Philadelphia	Philadelphia.	137	30.000	30.040	29.980	54.4	85	19	30	5	61.4	44.5	18.9	30.0	30	7.0	4	59.0	38.0	2.14	11	12	10	8	NW	NW	NW	U. S. Army, Sgt. Sig. Corps.		
Philadelphia	Signal Office.	130	30.017	30.048	29.983	54.4	85	19	30	5	61.4	44.5	18.9	30.0	30	7.0	4	59.0	38.0											

T. F. TOWNSEND, *Observer, Assistant.*

<sup>1</sup> Observations taken at 8 A. M. and 8 P. M.      <sup>2</sup> Observations taken at 12 Noon.



I.

	Lock Haven.	Mauch Chunk,	Meadville,	McConnellsburg,	New Castle.	Nisbet.
7	..	'11	..	..	..	..
2	..	'21	'49	'52	..	'50
4	..	'09	..	..	..	..
	..	*	'15	..	'48	..
	..	..	..	..	..	..
	..	..	..	..	..	..
	..	..	..	..	'08	..
	..	'05	..	..	..	'10
4	..	*	'55	1'72	'67	'30
	..	..	..	..	..	..
	..	'33	..	..	'05	..
	..	..	..	..	'22	'10
	..	'10	..	..	..	..
5	..	*	..	'34	'18	'70
	..	*	..	..	..	..
	..	*	..	..	..	..
	..	*	..	..	..	..
	..	*	..	..	'33	'10
	..	*	..	..	'07	'20
	..	*	..	..	..	..
	..	*	..	..	..	..
	..	*	..	..	..	..
	..	*	..	..	..	..
	..	*	..	..	..	..
	..	*	'34	..	'28	..
	..	..	..	..	..	..
7	..	'89	1'53	2'58	2'36	2'00

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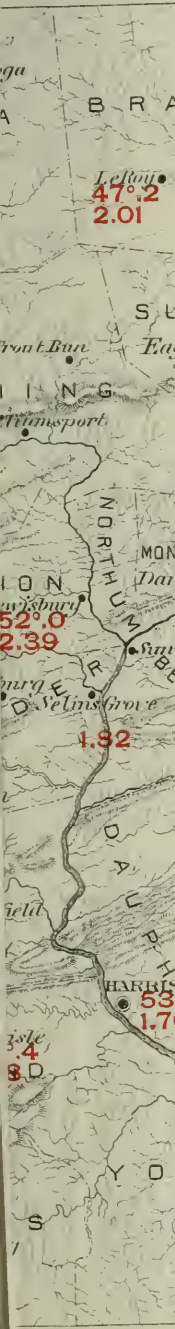
## PRECIPITATION FOR APRIL, 1891.

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\* No record.

† U. S. Signal Office.

T F 1.



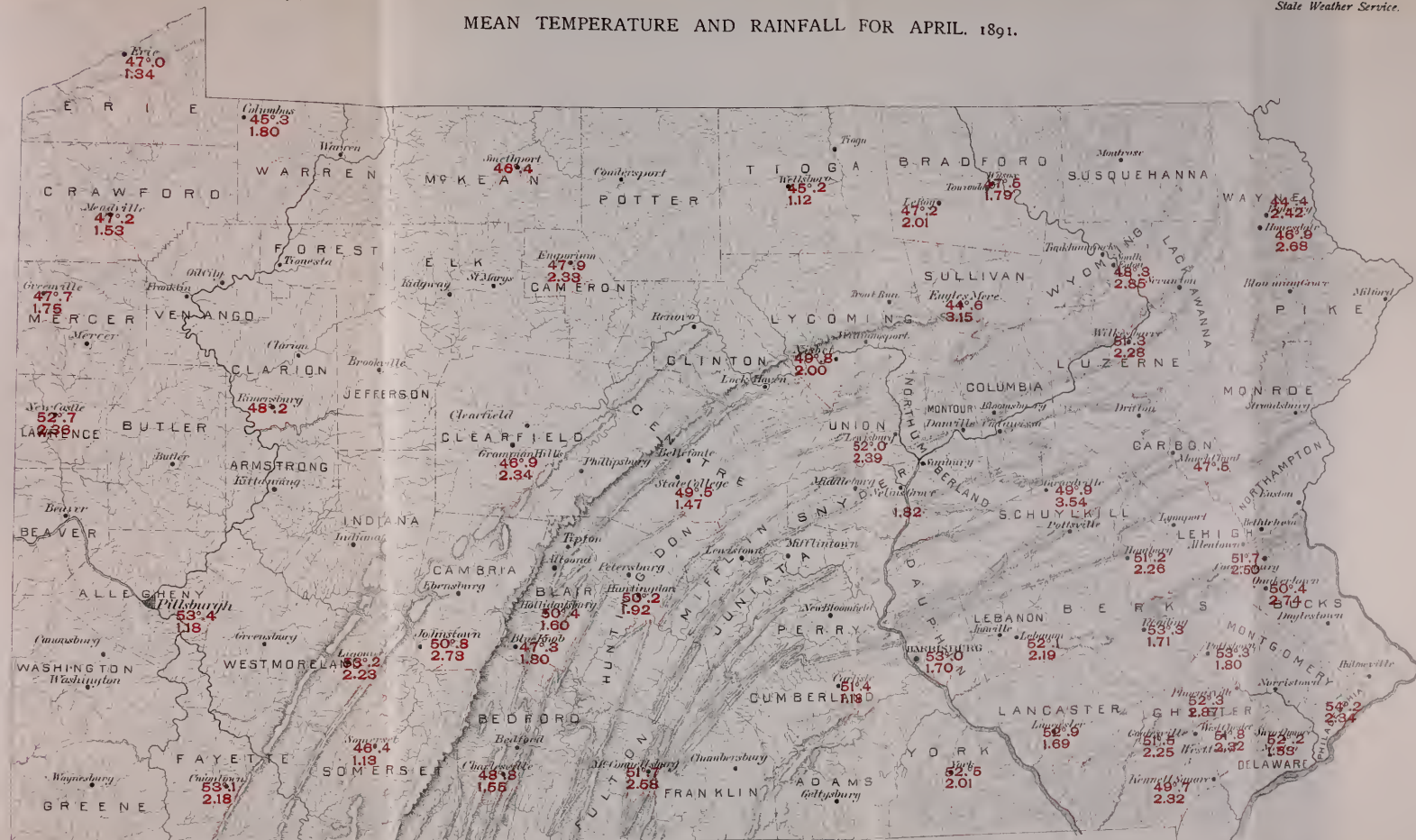
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# MEAN TEMPERATURE AND RAINFALL FOR APRIL, 1891.



PRECIPITATION.	NUMBER OF DAYS	WIND.
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[SUPPLEMENT.]

MONTHLY SUMMARIES OF THE REPORTS OF THE  
VOLUNTARY OBSERVERS OF THE PENNSYLVANIA  
STATE WEATHER SERVICE.—JUNE TO NOVEMBER, 1891.

Through the courtesy of the honorable Secretary of Internal Affairs we are enabled to include under the cover of this number of the JOURNAL, the monthly summaries of the reports of the voluntary observers of the Pennsylvania State Weather Service, from June to November, 1891, inclusive, with the addition of certain special reports by individual observers.

These summaries may be bound in their appropriate places in the volumes of the JOURNAL containing the months of August, 1891, to January, 1892, inclusive.

The publishers of the JOURNAL hope at some future day to supply the summaries necessary to fill up the gap from the last date forward until the regular monthly publication by the committee shall be renewed.

W. P. T.

	0	13	2	1	NW	SW	SW	J. M. DEY.
Some	10	8	7	15	SW	SW	SW	W. M. Schrock.
Sully	13	10	10	10	N	NW	SW	E. S. Chase.
Tloga	9	16	8	6	W	W	W	H. D. Deming.
Unio	11	14	9	7	NE	SW	SW	F. O. Whitman.
Warr	10	16	6	8	NW	NW	NW	Wm. Loveland.
Wash	9	17	9	14	.....	.....	.....	A. L. Runion, M. D.
Wayl	8	18	5	7	NW	NW	NW	Theodore Day.
West	13	14	9	7	NW	NW	NW	John Torrey.
Wyo	8	17	7	6	E	SW	SW	J. T. Ambrose.
York								Benj. M. Hall.
Del								Mrs. L. H. Grenewald.
								Wm. S. Morrison.

L. M. DEY, Observer in Charge.

THE WEATHER SERVICE STATIONS, 031, 032, 049, and LEWISBURG, 007.





*her Service for June, 1891.*

	PRECIPITATION.		NUMBER OF DAYS			WIND.			OBSERVERS.
	Snow fall.	Number of rainy days.	Clear.	Fair.	Cloudy.	PREVAILING DIRECTION.			
						7 a. m.	2 p. m.	9 p. m.	
Alle		10	11	7	12	NW		NW	Oscar D. Stewart, Ob. W'th. Bu.
Bedi		12	12	6	12	N	N	N	Miss E. A. G. Apple.
Berk		8	10	13	7	NW	N	NW	William Shipe.
Berk									Franklin Yeager.
Blai		11							Dr. Charles B. Dudley.
Blai		11	11	9	10	NW	NW	NW	A. H. Boyle.
Blai		12	16	5	9	E	W	E	Prof. J. A. Stewart.
Brac		11	14	6	10	SE	SE	SE	Charles Beecher.
Brac		11	13	8	9	SW	SW	SW	George W. T. Warburton.
Bucl									J. C. Hillsman.
Bucl		11	13	12	5	SW	SW	SW	J. L. Heacock.
Cam		14	8	10	12				E. C. Lorentz.
Cam		12	15	8	7	NW	NW	NW	T. B. Lloyd.
Carb		11	16	9	5	NW	NW	NW	John J. Boyd.
Cent		14	10	12	8	W	S	NW	Prof. Wm. Frear, Agr. Exp. Sta.
Ches		13	20	4	6	NW	NW	S	Jesse C. Green, D. D. S.
Ches		12	14	9	7	W	W	W	W. T. Gordon.
Ches		10	16	7	7	NW	SE		Benjamin P. Kirk.
Ches		13	19	4	7	NW	NW	SW	Knowles Croskey.
Clar		7	14	8	8	W	W	W	Prof. Wm. F. Wickersham.
Clar			14	9	7	W	W	W	Rev. W. W. Deatrick, A. M.
Clea									C. M. Thomas, B. S.
Clint		13	10	10	10	E	W	E, W	Nathan Moore.
Colu		13	11	10	9	W	W	W	Prof. John A. Robb.
Craw		10	15	7	7	N	S	N	Robert M. Graham.
Cuni		13	9	13	8	E	W	W	J. A. B. H. Metcalf.
Dauj		12	14	9	6	W	W	W	J. E. Pague.
Erie		8	11	5	14	W	W	W	Frank Ridgway, Ob. W'th. Bu.
Faye		11	15	9	6	SW	SW	SW	Prof. Susan J. Cunningham.
Fran									Peter Wood, Ob. W'th. Bu.
Fult		13	10	13	7	W	W	W	Wm. Hunt.
Hun		8	21	3	6	W	W	W	Miss Mary A. Ricker.
Indli									Thomas F. Sloan.
Lanc		10	10	7	13	S	E	E	Prof. W. J. Swgart.
Lawa		11	13	10	7	SW	SW	SW	Prof. S. C. Schmucker.
Lebo		12	15	9	6	SW	SW	NE	Lewis T. Lampe.
Lebi									Wm. T. Butz.
Luze		12	19	1	10	NE		SW	George F. Hayes, C. E.
Luzo		12	15	6	9	W	W	W	M. H. Boye, M. D.
McK		12	11	10	7	S	S	S	H. D. Miller, M. D.
Merc		13	15	8	7	S	S	S	A. W. Betterly.
Mont		7	20	5	5	W	W	W	J. S. Gibson, P. M.
Nort									Armstrong & Brownell.
Phils		7	15	7	8	SW	SW	SW	Prof. S. H. Miller.
Phils		8	14	9	7	NW	SW	SW	Charles Moore, D. D. S.
Schu		10	23	4	3	W	W	NW	Lerch & Rice.
Snyd		8	14	9	7	NW	SW	SW	Luther M. Dey, Ob. W'th. Bu.
Some		10	8	7	15	SW	SW	SW	John Comly.
Sull									E. C. Wagner.
Tlog		13	10	10	10	N	NW	SW	J. M. Boyer.
Unio		9	16	8	6	W	W	W	W. M. Schrock.
Warr		11	14	9	7	NE	SW	SW	E. S. Chase.
Wash									H. D. Deming.
Wayl		9	16	6	8	NW	NW	NW	F. O. Whitman.
West		8							Wm. Loveland.
Wyo		9	17	9	14				A. L. Runion, M. D.
York		13	14	5	7	NW	NW	NW	Theodore Day.
Yorl		13	14	7	7	NW	NW	NW	John Torrey.
Dela		8	17	7	6	E	SW	SW	J. P. Ambrose.

L. M. DEY, *Observer in Charge.*

— THE ABOVE TOWN OF NEW BRUNSWICK, 031, WYSONA, 049, and LEWISBURG, 007.

# MONTHLY SUMMARY OF REPORTS by Voluntary Observers of the Pennsylvania State Weather Service for June, 1891.

COUNTY.	STATION.	level (feet.)	BAROMETER REDUCED TO SEA LEVEL.			TEMPERATURE.										PRECIPITATION.		NUMBER OF DAYS.			WIND.			OBSERVERS.				
			Mean.	Highest.	Lowest.	Maximum.	Minimum.	Mean of maximum.	Mean of minimum.	Mean.	DAILY RANGE.			Relative humidity.	Dew point.	Total inches.	Snow fall.	Number of rainy days.	Clear.	Fair.	Cloudy.	PREVAILING DIRECTION.						
											Date.	Lowest.	Date.									Mean.	Greatest.		Date.	Least.	Date.	T. h. m.
Allegheny.	Pittsburg.	847	29.980	30.220	29.620	72.3	92	15	50	82.1	62.5	17.5	30	25	5	3.90		10	12	7	12	NW	N	NW	Oscar D. Stewart, Ob. Wth. Bu.			
Allegheny.	Charlestown.	1,360				68.7	91	15	40	79	54	35	41	28	6	4.42		12	12	8	12	NW	N	NW	Miss E. A. G. Apple.			
Allegheny.	Bedford.	880				70.4	104	17	42	8	83.6	57.2	26.4	81	14	6	2.23		8	10	13	7	NW	N	NW	William Sulpe.		
Allegheny.	Seneca.	280				72.5	95	15	48	8	82.5	62.5	10	30	27	7.73		11	11	9	10	SW	NW	NW	Franklin Venger.			
Allegheny.	Altoona.	1,181				72.5	95	15	48	8	82.5	62.5	10	30	27	6.59		11	11	9	10	SW	NW	NW	Dr. Charles B. Dudley.			
Allegheny.	Blue Knob.	2,500				68.7	98	16	39	6	82	54	21	45	28	8	6.30		12	18	14	6	W	W	W	Prof. J. A. Stewart.		
Allegheny.	Blair.	947				68.7	98	16	39	6	82	54	21	45	28	8	4.91		12	18	14	6	W	W	W	Charles Beecher.		
Allegheny.	Holidayburg.	718	29.950	30.250	29.695	66.2	98	15	33	6	80.4	52.1	25.3	49	9	7	21	11	14	6	10	SE	SE	SE	George W. T. Warburton.			
Allegheny.	Wysox.	1,400				66.2	98	15	33	6	80.4	52.1	25.3	49	9	7	21	11	14	6	10	SE	SE	SE	J. C. Hilsman.			
Allegheny.	Le Roy.					68.9	93	16	40	5	77	58.9	18.1	30	10	2	4.75		11	13	8	9	SW	NW	NW	J. L. Henshaw.		
Allegheny.	Forks of Nesheim.					68.7	95	15	50	8	82.5	62.5	10	30	27	4.75		12	11	9	10	SW	NW	NW	Ed. C. Lorenz.			
Allegheny.	Quakertown.	536	29.980	30.220	29.720	68.1	97	16	43	9	81.7	53.9	27.8	40	14	9	21	11	15	12	7	SW	SW	SW	T. B. Lloyd.			
Allegheny.	Johnstown.	1,184	30.006	30.272	29.646	67.9	92	14	39	8	82.5	55.2	29.4	45	14	10	19	12	15	8	7	SW	NW	NW	Prof. Wm. Frear, Agr. Exp. Sta.			
Allegheny.	Emporium.	1,680				67.9	92	14	39	8	82.5	55.2	29.4	45	14	10	19	12	15	8	7	SW	NW	NW	Jesse C. Green, B. D. S.			
Allegheny.	Mauch Chunk (27 days).	550				67.9	92	14	39	8	82.5	55.2	29.4	45	14	10	19	12	15	8	7	SW	NW	NW	W. T. Gordon.			
Allegheny.	Centre.	1,181	29.931	30.271	29.618	66.8	91	15	40	8	77.8	56.1	21.7	45	28	8	19	12	14	9	6	W	N	NW	Benjamin P. Kirk.			
Allegheny.	West Chester.	459	29.967	30.218	29.753	69.4	98	16	46	8	82.5	56.2	25.9	39	8	11	19	12	14	9	6	W	W	W	Knuckles Cronley.			
Allegheny.	Conestoga.	380				68.9	93	16	39	8	82.5	56.2	25.9	39	8	11	19	12	14	9	6	W	NW	SE	Prof. Wm. F. Wickersham.			
Allegheny.	Kennett Square.	215				68.9	93	16	39	8	82.5	56.2	25.9	39	8	11	19	12	14	9	6	W	NW	SE	Rev. W. W. Deitrick, A. M.			
Allegheny.	Phoenixville.	183	29.983	30.236	29.740	69.7	94	16	26	42	79.7	50.4	20.5	32	8	19	5	16	14	9	7	W	W	W	C. M. Thomas, B. S.			
Allegheny.	Westtown.	350	30.086	30.225	29.742	69.5	92	17	42	12	77.6	62.2	15.4	26	8	13	5	16	14	9	7	W	W	W	Nathan Moore.			
Allegheny.	Rimersburg.	1,630				69.9	97	15	48	5	77.6	62.2	15.4	26	8	13	5	16	14	9	7	W	W	W	Prof. John A. Robb.			
Allegheny.	Clarion.	1,500				67.9	90	14	36	7	79.7	60.1	19.6	36	9	14	6	17	13	10	10	E	W	E	Robert M. Graham.			
Allegheny.	Champion Hill.	1,450				68.9	97	15	42	8	81.3	55.4	25.9	40	14	6	17	13	10	10	10	E	W	E	Wm. T. Bate.			
Allegheny.	Lock Haven.	569				68.5	91	15	42	8	81.3	55.4	25.9	40	14	6	17	13	10	10	10	E	W	E	George K. Hayes, C. E.			
Allegheny.	Carlisle.	1,300				63.3	82	15	40	5	77	52.1	21.9	46	29	13	10	16	7	7	7	N	N	N	H. D. Miller, M. D.			
Allegheny.	Meadville.	480				69.4	97	16	43	9	82.5	56.2	25.9	39	8	11	19	12	14	9	6	W	W	W	A. W. Boye, M. D.			
Allegheny.	Carlisle.	1,300				69.4	97	16	43	9	82.5	56.2	25.9	39	8	11	19	12	14	9	6	W	W	W	H. D. Miller, M. D.			
Allegheny.	Harrisburg.	490	29.985	30.291	29.731	70.1	93	15	43	8	79.8	50.1	19.7	31	10	7	19	12	14	9	6	W	W	W	John A. Wagner.			
Allegheny.	Swarthmore College.	150				69.4	92	16	44	10	80.4	68.4	22.9	44	29	13	10	16	7	7	7	NW	NW	NW	K. C. Boyer.			
Allegheny.	Erie.	681	29.960	30.260	29.660	71.6	91	17	48	10	81.1	61.9	19.1	30	28	8	17	57	12	11	10	SW	SW	SW	Ed. C. Lorenz.			
Allegheny.	Uniontown.	1,004				71.6	91	17	48	10	81.1	61.9	19.1	30	28	8	17	57	12	11	10	SW	SW	SW	Prof. Susan J. Cunningham.			
Allegheny.	Chambersburg.	618				68.5	94	16	42	9	81.0	56.9	25.9	41	29	5	7	78.3	60.9	6.23	13	10	13	7	W	W	Peter Wood, Ob. Wth. Bu.	
Allegheny.	Franklin.	875				67.9	93	16	40	9	81.0	56.9	25.9	41	29	5	7	78.3	60.9	6.23	13	10	13	7	W	W	W	Wm. Hunt.
Allegheny.	McConnellsburg.	620				68.6	96	16	40	9	81.0	56.9	25.9	41	29	5	7	78.3	60.9	6.23	13	10	13	7	W	W	W	Miss Mary A. Ricker.
Allegheny.	Harrisburg.	1,500				68.6	96	16	40	9	81.0	56.9	25.9	41	29	5	7	78.3	60.9	6.23	13	10	13	7	W	W	W	Thomas F. Sloan.
Allegheny.	Indiana.	1,350				68.6	96	16	40	9	81.0	56.9	25.9	41	29	5	7	78.3	60.9	6.23	13	10	13	7	W	W	W	Prof. W. J. Schwartz.
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	Prof. S. C. Schaecker.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	Wm. T. Bate.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	George K. Hayes, C. E.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	A. W. Boye, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43	17	38	3	4.02		10	10	7	18	SE	SE	SE	H. D. Miller, M. D.	
Allegheny.	Lancaster.	474				72.8	94	15	34	12	81.5	64.4	27.8	43														

# PENNSYLVANIA STATE WEATHER SERVICE.

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## MONTHLY WEATHER REVIEW.

FOR MAY, 1891.

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*Prepared under the Direction of the Committee on Meteorology of the  
Franklin Institute.*

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HALL OF THE FRANKLIN INSTITUTE,  
PHILADELPHIA, May 31, 1891.

### TEMPERATURE.

The mean temperature of 52 stations for May, 1891, was  $57^{\circ}5$ , which is about  $2^{\circ}5$  below the normal, and  $1^{\circ}3$  below the corresponding month of 1890.

The mean of the daily maxima and minima temperatures  $69^{\circ}2$  and  $45^{\circ}4$  give an average daily range of  $23^{\circ}8$ , and a monthly mean of  $57^{\circ}3$ .

Highest monthly mean,  $61^{\circ}0$  at Philadelphia, S. O.

Lowest monthly mean,  $51^{\circ}0$  at Wellsboro.

Highest temperature recorded during the month,  $93^{\circ}0$  on the 10th at Carlisle and McConnellsburg.

Lowest temperature  $20^{\circ}0$  on the 4th at Dyberry.

Greatest local monthly range,  $64^{\circ}$  at Hollidaysburg and Dyberry.

Least local monthly range,  $48^{\circ}$  at Erie, S. O.

Greatest daily range,  $53^{\circ}$  at Hollidaysburg on the 10th.

Least daily range,  $2^{\circ}$  at Wellboro on the 18th.

From January 1, 1891, to May 31, 1891, the excess in temperature at Philadelphia was  $258^{\circ}$ , at Erie  $250^{\circ}$ , and at Pittsburgh  $222^{\circ}$ .

The warmest days of the month were the 10th and 11th. The coldest were the 4th, 5th, 6th, 7th and 17th.

### BAROMETER.

The mean pressure for the month,  $30^{\circ}07$ , is about  $\cdot 07$  above the normal. At the U. S. Signal Service Stations, the highest observed was  $30^{\circ}42$ , at Philadelphia, on the 20th, and the lowest  $29^{\circ}76$  at Philadelphia, on the 16th.

### PRECIPITATION.

The average precipitation,  $2^{\circ}12$  inches for the month, is a deficiency of about one and a quarter inches. The largest totals in inches (including melted snow) were Ligonier,  $4^{\circ}41$ , and Somerset,  $4^{\circ}16$ .

The least were Blue Knob,  $0^{\circ}31$ ; Wysox,  $0^{\circ}49$ , and Lewisburg,  $0^{\circ}67$ .

The largest monthly snowfall totals in inches were Columbus, 2'00; Wellsboro, 1'50, and Meadville, 1'50.

#### WIND AND WEATHER.

The prevailing wind was from the Northwest.

Average number: Rainy days, 8; clear days, 10; fair days, 10; cloudy days, 11.

#### MISCELLANEOUS PHENOMENA.

*Thunder-storms.*—Hamburg, 22d; Blue Knob, 20th, 22d, 30th; Hollidaysburg, 20th, 22d; Le Roy, 30th; Quakertown, 22d; Johnstown, 20th, 21st, 30th; Emporium, 20th, 31st; State College, 20th; Coatesville, 11th, 22d; Kennett Square, 3d, 22d; Westtown, 11th, 20th; Carlisle, 22d, 26th; Swarthmore, 11th, 20th, 22d, 31st; Huntingdon, 20th, 21st, 22d, 30th; Lancaster, 3d, 11th, 22d; Lebanon, 22d; Pottstown, 3d; Philadelphia, 11th, 20th, 22d; Girardville, 15th, 20th, 24th; Selins Grove, 20th, 22d; Somerset, 21st, 22d, 30th; Wellsboro, 20th, 30th; Lewisburg, 21st; Columbus, 20th, 22d; Ligonier, 20th, 21st, 30th; Meadville, 21st, 30th; York, 11th, 22d.

*Hail.*—Coatesville, 11th; Phoenixville, 11th; Selins Grove, 22d; Wellsboro, 6th.

*Snow.*—Hamburg, 6th; Blue Knob, 4th; Hollidaysburg, 6th; Le Roy, 5th, 6th; Johnstown, 6th; Emporium, 5th; Mauch Chunk, 6th; Rimersburg, 5th; Lock Haven, 5th; Carlisle, 6th; Harrisburg, 6th; Swarthmore, 6th; Huntingdon, 6th; Lancaster, 6th; Coopersburg, 6th; Wilkes-Barre, 6th; Greenville, 5th, 6th; Smethport, 4th, 5th, 6th; Philadelphia, 6th; Girardville, 4th, 6th; Selins Grove, 6th; Wellsboro, 5th; Lewisburg, 6th; Columbus, 5th; Dyberry, 6th; Ligonier, 6th; York, 6th; Grampian Hills, 5th; Eagles Mere, 5th.

*Frost.*—Pittsburg, 4th, 5th, 6th, 7th, 8th, 12th, 13th, 17th; Hamburg, 5th; Blue Knob, 4th, 5th, 7th, 8th, 12th, 13th, 17th, 18th, 19th, 27th; Hollidaysburg, 4th, 5th, 18th; Wysox, 2d, 3d, 4th, 5th, 6th, 8th, 17th, 19th, 27th; Le Roy, 4th, 5th, 6th, 17th, 27th; Quakertown, 2d, 4th, 5th, 6th, 7th, 8th, 10th, 13th, 15th, 16th, 17th, 18th, 19th, 23d, 27th, 28th; Johnstown, 4th, 5th, 8th, 17th, 18th; Emporium, 4th, 13th, 17th, 18th, 27th; State College, 4th; Coatesville, 4th, 5th, 6th; Phoenixville, 4th; Westtown, 6th; Rimersburg, 4th, 5th, 12th, 13th, 17th, 18th, 27th; Lock Haven, 4th, 5th, 6th; Carlisle, 4th; Swarthmore, 6th; New Castle, 4th, 5th, 7th, 8th, 12th, 13th, 17th, 18th, 27th; Lebanon, 4th; Coopersburg, 4th, 5th, 6th; Wilkes-Barre, 4th, 5th, 19th; Nisbet, 4th, 27th; Pottstown, 4th; Selins Grove, 2d, 4th, 5th, 18th, 27th; Somerset, 4th, 5th, 6th, 8th, 17th, 18th, 19th, 28th; Wellsboro, 2d, 3d, 4th, 5th, 6th, 7th, 8th, 12th, 13th, 14th, 17th, 18th, 27th; Lewisburg, 5th; Dyberry, 2d, 4th, 5th, 6th, 7th, 8th, 13th, 14th, 17th, 18th, 19th, 27th, 28th; Honesdale, 1st, 4th, 6th, 17th, 19th, 27th; South Eaton, 2d, 4th, 5th, 6th, 17th; York, 4th, 5th, 17th; Greenville, 5th, 7th, 12th, 13th, 17th, 27th; Columbus, 4th, 5th, 6th, 7th, 8th, 12th, 13th, 14th, 15th, 17th, 18th, 19th, 27th; Meadville, 4th, 5th, 7th, 12th, 13th, 17th, 27th; Grampian Hills, 4th, 5th, 8th, 12th, 17th, 18th, 27th; Eagles Mere, 27th.



*Sleet*.—Blue Knob, 4th, 6th; New Castle, 5th; Wellsboro, 6th; Ligonier, 4th.

*Corona*.—Blue Knob, 19th; Lebanon, 1st, 2d, 8th, 13th, 14th, 15th, 16th, 17th, 19th, 20th, 21st, 22d; Somerset, 22d.

*Solar Halos*.—Hollidaysburg, 2d; Le Roy, 1st, 2d, 13th, 25th, 27th, 28th; State College, 16th; Rimersburg, 27th; Huntingdon, 13th; Philadelphia, 13th, 27th; Dyberry, 23d, 25th, 28th; Eagles Mere, 20th, 23d, 27th.

*Lunar Halos*.—Blue Knob, 18th; Phoenixville, 13th; Carlisle, 13th, 14th; Lancaster, 20th; Somerset, 19th, 21st; York, 19th; Meadville, 16th, 19th, 20th.

*Meteors*.—State College, 5th.

*Parheli*as.—Dyberry, 2d; Eagles Mere, 23d.

#### WEATHER FORECASTS.

Percentage of local verifications of weather and temperature signals as reported by displaymen for May, 1891:

Weather, 85 per cent.

Temperature, 91 per cent.

#### TEMPERATURE AND WEATHER SIGNAL DISPLAY STATIONS.

<i>Displayman.</i>	<i>Station.</i>
U. S. Signal Office, . . . . .	Philadelphia.
Wanamaker & Brown, . . . . .	"
Pennsylvania Railroad Company, . . . . .	"
Continental Brewing Company, . . . . .	"
Samuel Simpson, . . . . .	"
B. T. Babbitt, . . . . .	"
Western Meat Company, . . . . .	"
Neptune Laundry, . . . . .	"
C. W. Burkhart, . . . . .	Shoemakersville.
A. N. Lindenmuth, . . . . .	Allentown.
C. B. Whitehead, . . . . .	Bradford.
Thomas F. Sloan, . . . . .	McConnellsburg.
J. H. Fulmer, . . . . .	Muncy.
Capt. A. Goldsmith, . . . . .	Quakertown.
Frank Ross, . . . . .	Oil City.
Lerch & Rice, . . . . .	Bethlehem.
Signal Office, . . . . .	Erie.
J. R. Raynsford, . . . . .	Montrose.
E. P. Wilbur & Co., . . . . .	South Bethlehem.
Agricultural Experiment Station, . . . . .	State College.
Signal Office, . . . . .	Pittsburgh.
<i>New Era</i> , . . . . .	Lancaster.
D. G. Hurley, . . . . .	Altoona.
J. E. Forsythe, . . . . .	Butler.
Steward M. Dreher, . . . . .	Stroudsburg.
State Normal School, . . . . .	Millersville.

<i>Displayman.</i>	<i>Station.</i>
E. C. Wagner, . . . . .	Girardville.
L. H. Grenewald, . . . . .	York.
J. E. Pague, . . . . .	Carlisle.
C. L. Peck, . . . . .	Coudersport.
H. D. Miller, . . . . .	Drifton.
M. Tannehill, . . . . .	Confluence.
S. C. Burkholder, . . . . .	Pollock.
Robt. M. Graham, . . . . .	Catawissa.
Henry F. Bitner, . . . . .	Millersville.
A. M. Wildman, . . . . .	Langhorne.
G. W. Klee, . . . . .	Chambersburg.
A. Simon's Sons, . . . . .	Lock Haven.
<i>Raftsmen's Journal,</i> . . . . .	Clearfield.
R. C. Schmidt & Co., . . . . .	Belle Vernon.
Chas. B. Lutz, . . . . .	Bloomsburg.
E. C. Lorentz, . . . . .	Johnstown.
W. M. James, . . . . .	Ashland.
E. J. Sellers, . . . . .	Kutztown.
H. M. Kaisinger, . . . . .	Hartsville.
Foulk & Co., . . . . .	Milford.
William Lawton, . . . . .	Wilmington, Del.
Wister Heberton & Co., . . . . .	Germantown.
E. W. Merrill, . . . . .	North East.
Frank Ridgway, . . . . .	Harrisburg.
Powell Bros., . . . . .	Shadeland.
J. C. McCloskey, . . . . .	Lock Haven.
Charles Petersen, . . . . .	Honesdale.
Armstrong & Brownell, . . . . .	Smethport.
Frank Loomis, . . . . .	Troy.

## STATE WE

	Relative Humidity.	Dew Point.
Al	62.3	44.2
Be	..	..
Ba	..	..
Bl	41.9	38.5
Bl	..	..
Bl	65.6	45.7
Br	69.2	44.9
Bt	..	..
Bt	..	..
Bt	69.9	47.3
C	81.1	49.2
C	..	..
C	..	..
C	..	..
Cl	62.1	42.8
Cl	65.0	46.7
Cl	..	..
Cl	..	..
Cl	73.4	49.8
Cl	..	..
Cl	..	..
Cl	..	..
Cl	..	..
C	..	..
C	..	..
C	..	..
C	77.0	45.0
C	79.0	51.0
D	61.4	44.8
D	..	..
E	68.0	49.0
F	73.0	45.0
F	..	..
F	..	..
H	63.4	35.8
In	..	..
In	..	..
L	..	..
L	72.0	48.5
L	..	..
L	70.2	47.9
L	..	..
L	..	..
L	..	..
L	..	..
M	..	..
M	..	..
M	83.0	45.4
M	73.0	50.2
N	..	..
N	..	..
P	..	..
P	63.0	45.6
P	77.0	50.2
S	..	..
S	79.6	51.2
S	78.3	48.2
S	69.1	43.2
T	64.2	38.6
U	..	..
W	60.0	36.1
W	..	..
W	..	..
W	70.4	44.2
W	72.0	50.2
W	..	..
Y	74.1	50.2

# MONTHLY SUMMARY OF REPORTS BY VOLUNTARY OBSERVERS OF THE PENNSYLVANIA STATE WEATHER SERVICE FOR MAY, 1891.

COUNTY.	STATION.	Elevation above Sea Level (feet).	BAROMETER REDUCED TO SEA LEVEL.			TEMPERATURE.										PRECIPITATION.				NUMBER OF DAYS.			WIND.			OBSERVERS.					
			Mean.	Highest.	Lowest.	MAXIMUM.				MINIMUM.		Mean of Maximum.	Mean of Minimum.	DAILY RANGE.					Total Inches.	Total Snowfall During Month.	Number of Days.	Clear.	Fair.	Cloudy.	7 A. M.		9 P. M.	9 P. M.			
						Mean.	Highest.	Date.	Lowest.	Date.	Mean of Maximum.			Mean of Minimum.			Mean.	Greatest.											Date.	Least.	Date.
Allegheny,	Pittsburgh,	847	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Oscar D. Stewart, Sgt. Sig. Corps.			
Berkley,	Charlesville,	1,300	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Miss E. A. Apple.			
Berkley,	Hamburg,	380	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	William Shippe.			
Berkley,	Reading,	280	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Franklin Yager.			
Blair,	Altoona,	1,181	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Dr. Charles B. Dudley.			
Blair,	Blue Knob,	2,500	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	A. H. Boye.			
Blair,	Hollidaysburg,	715	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. J. A. Stewart.			
Bradford,	Wysox,	1,400	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Charles Becher.			
Bradford,	Le Roy,	1,400	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Geo. W. T. Warburton.			
Bucks,	Forks of Nesaminy (39 days),	1,400	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	E. C. Lorentz.			
Bucks,	Quakertown,	536	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	J. L. Heacock.			
Cambria,	Johantown,	1,184	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	T. B. Lloyd.			
Cameron,	Emertown,	1,020	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	John J. Boyd.			
Carbon,	Mauch Chunk (23 days),	550	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. Wm. Frear.			
Centre,	Agricultural Experiment Station,	1,191	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	James C. Green, D.D.S.			
Chester,	West Chester,	455	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	W. T. Gordon.			
Chester,	Croftsville,	380	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Henry P. Kirk.			
Chester,	Kennett Square,	375	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Samuel Cressley.			
Chester,	Phoenixville,	183	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. Wm. F. Wickersham.			
Chester,	Westtown,	350	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Rev. W. W. Deatrick, A.M.			
Clarion,	Rimersburg,	1,500	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	C. M. Thomas, B.S.			
Clarion,	Clarion,	1,500	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Nathan Moore.			
Clearfield,	State Normal School,	1,530	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. John A. Robb.			
Cleone,	Grampian Hills,	1,450	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Robert M. Graham.			
Cleone,	Lock Haven,	550	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	J. B. H. Metcalf.			
Columbia,	Catawissa,	491	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Frank Ridgway, Sgt. Sig. Corps.			
Crawford,	Mendville,	1,250	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. Susan J. Cunningham.			
Cumberland,	Carlisle,	480	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Peter Wood, Sgt. Sig. Corps.			
Dauphin,	Harrisburg,	361	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Wm. Hunt.			
Delaware,	Swarthmore College,	190	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. Susan J. Cunningham.			
Delaware,	Swarthmore College,	190	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Peter Wood, Sgt. Sig. Corps.			
Erie,	Erie,	681	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Wm. Hunt.			
Fayette,	Latrobe,	1,000	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Miss Mary A. Ricker.			
Franklin,	Chambersburg,	681	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. W. J. Swigart.			
Franklin,	Wilkes Female College,	681	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Prof. S. C. Schmecker.			
Fulton,	McConnellsburg,	875	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Lewis T. Lampe.			
Huntingdon,	Huntingdon,	690	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	E. C. Wacker.			
Indiana,	The Normal College,	580	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Geo. W. Hayes, C.E.			
Indiana,	Indiana,	1,350	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	M. H. Boye.			
Lancaster,	State Normal School,	1,350	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	H. D. Miller, M.D.			
Lancaster,	Lancaster,	1,350	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	John S. Gilman, P.M.			
Lawrence,	Franklin and Marshall College,	411	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Arnold & Brownell.			
Lebanon,	Lebanon,	932	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	John S. H. Miller.			
Lebanon,	Lebanon,	474	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Chas. Moore, D.D.S.			
Lehigh,	Lebanon,	520	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Lerch & Re.			
Lehigh,	Coopersburg,	520	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	C. M. Thomas, B.S.			
Lerners,	Drifton Hospital,	1,665	30°100	30°320	29°860	58.8	84	10	34	17	68.9	48.8	20.1	34.0	19	5.0	28	62.3	44.2	3.23	10	8	8	15	W	W	W	Nathan Moore.			
Lerners,	Drifton Hospital,	1,665	30°100	30°320	29°860	58.8																									

	Altoma	McConnellsburg.	New Castle.	Nisbet.	Ottsville.	Philadelphia.†	Philadelphia.
1	.	.	'28	.	.	.	.
2	.	.	.	.	.	.	.
3	.	'18	.	'20	'89	'50	'6
4	.	.	.	.	.	.	.
5	.	.	'02	.	.	.	'6
6	.	.	.	.	.	.	.
7	.	.	.	.	.	.	.
8	.	.	.	.	.	.	.
9	.	.	.	.	.	.	.
10	.	.	.	.	.	.	.
11	.	.	'01	.	.	'08	'6
12	.	.	.	'10	'05	.	.
13	.	.	.	.	.	.	.
14	.	.	.	.	.	.	.
15	.	.	.	.	'10	'23	.
16	.	.	.	.	'73	'20	.
17	.	.	.	.	.	.	.
18	.	.	.	.	.	.	.
19	.	.	'05	.	.	.	.
20	.	.	'02	.	.	'15	.
21	'1	'25	'08	'40	.	.	.
22	'0	'43	'19	'50	'42	'05	.
23	'1	'16	'14	.	.	'11	.
24	'0	.	.	.	.	'04	.
25	.	.	.	.	.	'16	.
26	.	.	.	.	.	.	.
27	.	.	.	.	.	.	.
28	.	.	.	.	.	.	.
29	'1	'56	'63	'10	'77	'14	.
30	'0	'13	.	.	.	'03	.
31	'1	.	.	.	.	.	.
	1'9	3 1'71 2'42	1'20,3'01	1'74 2'			

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### PRECIPITATION FOR MAY, 1891.

Altoona	Bethlehem	Blue Knob	Brown's Luck	Canonsburg	Carlisle	Catawissa	Chambersburg	Charlottesville	Clarton	Coatesville	Columbus	Coopersburg	Doylstown	Drifton	Dyersburg	Eagles Mere	Emporium	Erie	Forks of Nehalem	Frederick	Gap	Grampian Hills	Greenville	Hamburg	Harrisburg	Hollidaysburg	Honesdale	Huntingdon	Indiana	Johnstown	Kennett Square	Lancaster	Lansdale	Lebanon	Le Roy	Lewistown	Ligonier	Lock Haven	Mauch Chunk	Meadville	McConnellsburg	New Castle	Nesbit	Ottisville	Philadelphia	Philadelphia	Pennsville	Pittsburgh	Pont Piquant	Pottstown	Quakertown	Radegast	Scranton	Selma Grove	Smith's Corner	Somerset	South State	State College	Swanton	Uniontown	Wellbore	West Chester	Weston	Wilkes-Barre	Wysox	York																																	
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64	65	66	67	68	69	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	100
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